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The Problem of the Association of Acid and Basic Rocks in Central Complexes.

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ONE of the outstanding problems of petrogenesis at the present time is that offered by the remarkable association of sharply contrasted acid and basic rocks (e.g. granite-gabbro ; granophyre-dolerite ; pitchstone-tholeiite ; and rhyolite-basalt) in igneous complexes such as those of the British Tertiary Province and those of the great lopoliths of Duluth and Sudbury in North America and the Bushveld in South Africa. The contrast was first recognized in the lavas of Iceland by Bunsen (1)[1] and it led him to the conception that two fundamental magmas, respectively acid and basic, were concerned in the genesis of the igneous rocks of Iceland and similar provinces elsewhere. Bunsen's view, however, has had little influence in the development of petrological philosophy. The petrologists of the Geological Survey in this country, and Bowen and others in North America, have assembled a very weighty and reasonable mass of field and laboratory evidence supporting the hypothesis that the acid rocks are residual products arising from the crystallization-differentiation of basaltic magmas. In the recently published *Ardnamurchan Memoir* (2), for example, it is claimed that as a result of the early extraction from the Plateau Magma of olivine, pyroxenes, basic plagioclase, and iron ores, the residual magma would reach a composition " which would find expression as quartz-doleritic rocks with an acid mesostasis capable of mechanical separation and a separate existence as acid lavas or intrusions " (p. 95). Bunsen's two magmas are thus regarded as successive products from a single parent stock.

[1] Numbers in parentheses refer to bibliography at end of paper.

There is nevertheless a simple alternative hypothesis which has for many years seemed to me to be preferable, involving the production of acid magma by the refusion or palingenesis of pre-existing rocks of granitic composition (3). The rival views are not necessarily mutually exclusive, and it would be foolish to deny that in appropriate circumstances the crystallization of excess olivine and its separation from the parental magma would ultimately lead to the production of an oversaturated magma. It is my opinion, however, and I wish in this paper to give my reasons for holding it, that in central complexes (such as those of Mull and Ardnamurchan) the circumstances are *not* appropriate to the production of acid magma by differentiation, but that, on the contrary, the differentiation processes there involved tend towards the generation of residual magmas of trachytic or syenitic composition.

If acid magma be a normal derivative of basaltic magma, as is reasonably urged on the evidence of rock associations in the central complexes of the continents, then similar associations should be at least equally common in the central complexes of the Pacific Islands, where the presumed parent (basalt) is dominant. But within the oceanic province bounded by the andesitic circum-Pacific belt rhyolites and granitic types generally are totally lacking. The facts are too striking to be ignored. In the latest review of the petrography of the Pacific islands Lacroix writes (4): "Il parait bien établi que dans aucune des îles étudiées il n'existe de roches éruptives de profondeur et de sédiments anciens ou de schistes crystallins, soit en place, soit même à l'état de blocs projetés par les explosions volcaniques. J'ai montré, en effet, que le prétendu *granite* cité par Ellis à Bora-Bora est une *dolérite* passant au gabbro, intrusive dans un basalte de même composition. Il n'est pas douteux qu'il en soit de même pour le granite signalé par le même auteur dans l'île voisine de Maupiti. J'ai décrit antérieurement avec les plus expresses réserves, un bloc de granite, recueilli sur la côte Nord de Taiarapu ; des recherches faites, depuis lors, ont montré qu'il s'agit là d'un bloc apporté par quelque bateau."

This remarkable and significant contrast between continental and oceanic complexes can be correlated with a similar contrast in crustal composition. The continents are characterized by a "granitic" layer averaging at least 10 or 12 km. in thickness, whereas the Central Pacific has no such layer. Where a central complex develops through the "granitic" layer the acid-basic association is commonly found, and micropegmatite is a characteristic interstitial constituent of innumerable quartz-dolerites. Where, on the other hand, otherwise similar complexes develop through the sial-free oceanic floor of the Pacific the acid-basic association is never found. The common oceanic association is oceanite—ankaramite—olivine-basalt—trachybasalt—trachyte (or a corresponding felspathoidal series), but this association is found in continental as well as oceanic complexes. The combined evidence

of world petrography and seismology thus supports the two generalizations :

(*a*) that the normal world-wide differentiation of basaltic magma is towards the generation of trachyte or its equivalents and relations ; and

(*b*) that associated granitic magmas are produced only where pre-existing rocks of the sial suite are present.

These in turn suggest the hypothesis that in acid-basic associations the acid magma is essentially the product of the partial or complete refusion of the granitic rocks locally available.

This hypothesis can, I think, be maintained on more direct grounds with the aid of some of the conclusions that are conveniently summarized by Dr. H. H. Thomas in the *Ardnamurchan Memoir*. On page 63 we read : " The origin of composite sills and dykes has always been a difficult problem, a final solution of which has not yet been found. Judging by the form and relations of the rocks involved, however, there appears to be no reasonable doubt of the simultaneous availability of two compositionally different magmas and of their coexistence in the same magma-reservoir, one magma following upon the other along the same fissure after a very short interval of time, before the first had an opportunity to solidify completely. . . . Dr. Harker, on the evidence of the composite intrusions of Skye, considered it beyond doubt that the magmas responsible coexisted in subterranean reservoirs, and that the lighter acid magma overlay the denser basic one."

The structure of a composite dolerite–quartz-porphyry dyke in Iceland has also provided Guppy and Hawkes with a convincing proof of the coexistence of acid and basic magmas (5).

Returning to pages 56 and 57 of the *Ardnamurchan Memoir*, we find the following most significant statements in the course of a general account of the remarkable intrusions known as cone-sheets. " Calculations from the inclinations of the sheets bring out the interesting fact that in all the Tertiary centres where such sheets are developed the foci were situated at a depth of about 3 miles below the present surface. It also follows that such foci must have been located within or upon the surface of a reservoir capable of supplying magma to fill the fissures. . . . The idea of the development of cupolas on the main magma basin seems necessary to explain the origin of cone-sheets and ring-dykes. Such cupolas are assumed to be steep dome-like or paraboloidal upward extensions of the main magma-reservoir into relatively cool regions of the crust."

This verbal picture of the form and position of the magma reservoir seems the only one consistent with the observational evidence. Since the cupola extends to within 3 miles, or say 5 km. of the present surface, it follows from our knowledge of the nature of the continental crust that the upper 7 km. or so of the cupola lies in the sial rocks of the upper layer, the thickness of which in a comparable region is estimated by Jeffreys at about 12 km. (6).

How far down the cupola may extend before merging into the regional magmatic layer whence the first magmas were derived is still a matter for conjecture. Nevertheless, no error of principle would appear to be involved in the assumption that the cupola rises from a level near the base of the intermediate layer, that is, from a depth of about 30–40 km. below the surface. The depth of the cupola may be greater, but it can hardly be less. The intermediate layer, as far as can be judged from geological and seismological evidence, appears to consist of material of basaltic composition, possibly in the state of amphibolite or granulite (7). Combining the form of the cupola with the structure of the crust, we thus arrive at the spatial relations represented in cross section by Fig. 1.

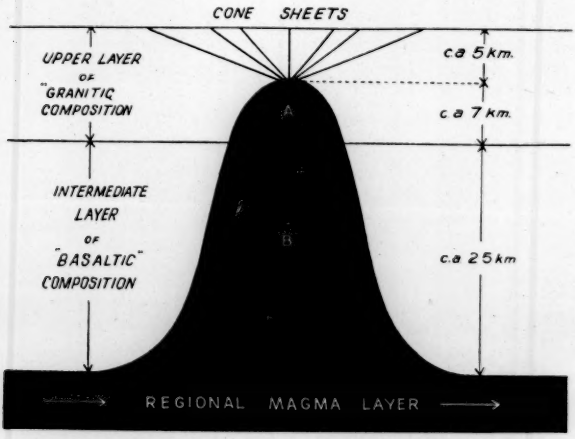

FIG. 1.—Idealized representation of a cupola-shaped magma reservoir showing its spatial relationship to the intermediate and upper layers of the continental crust.

To account for the origin of the cupola we have to explain the ascent of fluid conditions high into the crust, and consider what has happened to the rock material of the upper and intermediate layers that had previously filled the space A and B occupied by the cupola. The basic material, B, may have been stoped out or fused by the continued action of convective circulation as described below. Of the acid rock material, A, we may safely conclude that it was not mechanically pushed out of the way by lateral or upward pressure to any extent approaching 7 km. If part of the material of A sank piecemeal, after being liberated by stoping, it would sooner or later be fused, and would either rise again to its appropriate level in the

cupola or would contaminate the basic magma so that hybrids or such rocks as quartz-dolerites would be produced. If the material A did not sink, then it must have become wholly or partly fused *in situ*. Thus in either case it appears inevitable that much of the original material A must have subsequently passed into the magmatic state. Granting this, an adequate source is available for the acid magma that has demonstrably accumulated towards the top of the cupola when extrusions from a central complex are initiated.

Very little attention appears to have been given to the effects of heat transfer due to convective circulation in fusing crustal materials. That the effect must be very considerable is shown by comparing the thermal gradient maintained by convection in a magmatic reservoir with the much steeper gradient expressing the rise of fusion point with pressure or depth. The two gradients are plotted in Fig. 2. The fusion point for basaltic magma increases at the rate of about 3° C. per km., whereas in cooling by convection the gradient which the circulation tends to establish is a downward increase of about 0·3° C. per km. (8). The exact figures here matter less for our immediate purpose than the fact that the former rate of increase is about ten times the latter.[1]

Let us suppose that the composition of the first erupted plateau-magma (free from phenocrysts) was such that its temperature of complete fusion at a depth of about 35 km. was, say, 1205° C. (P in Fig. 2). Then at the surface, *r*, the fusion point would be about 1100° C. When the roof of the growing cupola had reached to within 15 km. of the surface, then at its top, Q or *q*, the fusion point would be about 1145° C. But the actual temperature near Q would be higher than this as a result of convection within the magma enclosed by the cupola. Under ideal circumstances the convection gradient PQ in a cupola 20 km. high would tend to maintain a temperature only 6° C. lower at the top Q than at the bottom P. The temperature at the level *Qq* would therefore tend constantly to approach 1199° C., i.e. 54° C. higher than the fusion point *q*. If the initial magma at 35 km. had a temperature above its fusion point, or came from a greater depth, this difference of 54° C. would be correspondingly increased. Thus, at Q in Fig. 3, a steady supply of heat from below would impinge against the roof. This would be available for supplying latent heat and fusing the rock material of the roof and sides nearby, and the augmented magma and liberated crystals would be carried down with the

[1] It is not to be expected that the fusion point-pressure gradient will continue in depth as a straight line. Work on the compressibilities of solids and liquids at high pressures indicates that with increasing depth the difference in specific volume between solid and liquid phases of the same substance becomes gradually smaller. The gradient referred to will therefore tend to flatten out with depth. Nevertheless, within the intermediate and upper layers, which are alone considered here, no error of principle is involved in the numerical assumptions that have been adopted for the sake of making a more concrete demonstration.

return peripheral circulation along or parallel to the enclosing walls. In this way the roof of the cupola would be gradually extended upwards so long as the supply of heat was maintained. By the term "liberated" crystals three types of crystals are connoted :—

(*a*) Xenocrysts set free from the walls by the first eutectic fusion-product whereby the original rock would be marginally and internally fritted.

(*b*) Crystalline products due to the thermal reconstitution of parts of the wall-rocks, these products being equivalent to phases with which the magma would locally and temporarily be saturated.

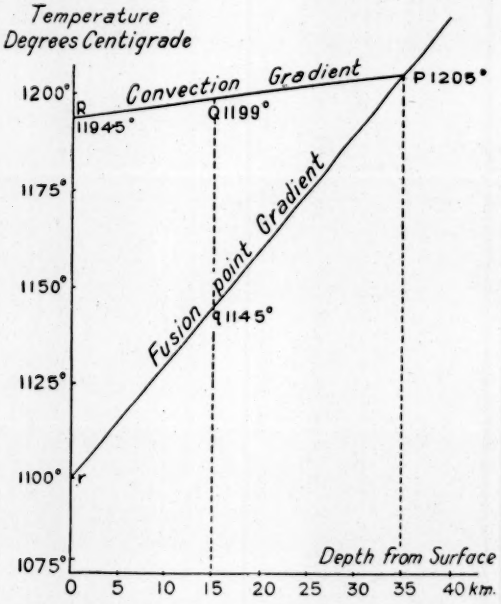

FIG. 2.—To illustrate the upward transfer of heat brought about by convective circulation.

(*c*) Precipitated crystals from the augmented magma corresponding, as in (*b*), to those phases with which the magma would be saturated. See Bowen's masterly treatment of this topic in his *Evolution of Igneous Rocks*, 1928, chap. x.

Bowen considers that the effect of "superheat" has been greatly exaggerated, although he mitigates the geological force of his statement by showing, in the light of his *reaction principle*, that even thermally saturated magmas "may produce very marked effects in the way of incorporation of foreign material" (9, p. 523). Bowen, however, has not taken into consideration the heat transport due to convection currents, and on this account his judgment of the

case for magmatic superheat, at least in the cupola-reservoirs here under consideration, must be regarded as incomplete. His application of the reaction principle to the problem of assimilation nevertheless reveals an important ancillary mechanism that must operate in cupola development (9).

Here we may pause to consider how the cupola itself originated. If the initial magma basin were a stationary pool situated below the site of the future central complex, and if its density were lower than that of the surrounding crust, then the column above the pool would tend to be heaved up a little. Alternatively, the application

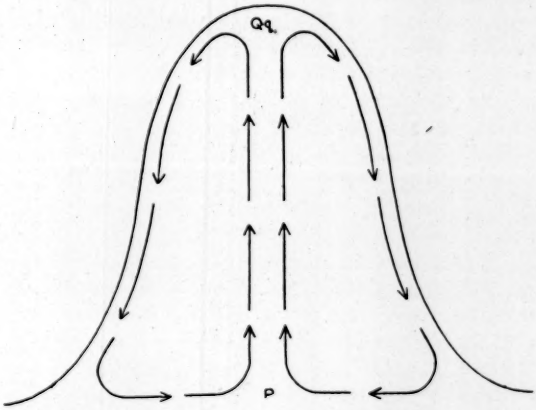

FIG. 3.—Idealized representation of the convective circulation in a cupola-shaped magma reservoir, to illustrate the upward transfer of heat from P to Q.

of external pressure, such as orogenic compression or isostatic "plunger-action", would cause the magma to raise its roof at any point where the vertical resistance was locally at a minimum. The growth of salt-domes appears to be a process similar to that here envisaged. Thus, for one reason or another, the roof of the pool would probably be domed from the start, and convective circulation, beginning slowly, would gradually promote an upward growth of the magma reservoir, the action being concentrated at the highest part and so leading to the development of the paraboloidal form. As indicated by Fig. 2, crystallization would begin first towards the bottom of the magma basin, and as cooling went on, the magma would rise on a floor of its own crystal accumulations (10). This would continue until the available excess of heat was balanced by the increasing loss due to endothermic processes and conduction through the roof and sides of the rising cupola.

More probably, however, the initial magma basin would not be stationary, but would be slowly moving. In this case the crystalline

roof would tend to have the form of a shallow trough (upside down) over the site of any local stream of maximum velocity. Superimposed upon this effect would be another based on the general fact that when one medium moves over another the surface between the two media tends to develop a wave-like form with the crest-lines at right angles to the direction of movement (11).

The crest-line of the magmatic stream-trough would consequently be sinusoidal rather than horizontal. The highest point reached by this interference effect would then define the focus from which a cupola could develop upwards.

Irregularities of the magmatic roof due to possible lateral variation in the structure or composition of the deep-seated crust might also provide dimples from which cupolas could grow by convective heating, and in any case such variations would control the loci of stream-troughs and wave-surfaces. Structural control of this kind appears to be indicated by the distribution of the chief Tertiary centres of Britain along a roughly north and south line from Skye to Carlingford. Dr. Thomas remarks in the *Ardnamurchan Memoir* (p. 54) that this alignment " appears to be more than a matter of coincidence, and points to a definite determining line of crustal weakness, unrelated to the direction followed by the majority of the dykes that form the respective swarms ". The direction so indicated, which on an average is from N.W. to S.E., is probably a surface expression of that followed by at least the later magmatic streams of the Tertiary cycle. The migration of the Mull and Ardnamurchan centres towards the north-west or south-west suggests that the magma-streams came from a westerly direction. This deduction is further supported by the perceptible fanning out of the Mull Swarm from Mull to the North of England coast.

However this may be, some movement of the deep-seated magma appears to be indicated, and consequently there would be a far greater supply of heat available for the upward growth of a cupola than if the initial magma basin had been a stationary pool.

Many cupolas have developed through the basic intermediate layer and well beyond into the upper or sial layer. When the roof of a cupola reaches the base of the sial the latter will necessarily begin to fuse if heat continue to be carried up by convection. There is ample evidence that such fusion would occur. Shepherd and Greig have shown experimentally that granite can be converted into glass at a temperature well below that at which basalt begins to show any change (12). Thomas has recorded from a sheet of craignurite in Mull a xenolith of pegmatite in which glass had developed along the margins of the felspars, and between felspars and quartz (13). Similar indications of preferential fusion between adjacent crystals of quartz and felspar in an Icelandic granite have been described by Hawkes (14), and I have recently found the same features in an ejected block of granodiorite-gneiss from the crater of Katwe in Uganda.

The phenomena attending the fusion of a deep-seated rock which has not a unique melting point have already been discussed in a former paper (9). Here it will suffice to point out that once a small body of acid magma has been produced it will in turn acquire a convective circulation in consequence of the heat brought up to it from the underlying basaltic magma. The directions of the currents in the acid magma will be in part controlled by those in the basic magma as illustrated in Fig. 4. The process will continue, as before, until the amount of heat lost to the surface by endothermic processes and by conduction from the top of the zone of fusion is equal to that brought up from below.[1]

The ultimate result provides the required conditions—coexistence of acid and basic magmas in a cupola-shaped reservoir—already

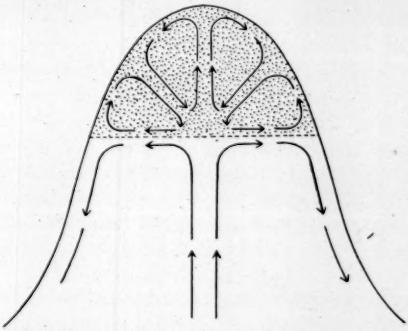

FIG. 4.—Idealized representation of the convective circulation in a cupola that has ascended into the " granitic " layer. The acid magma is indicated by dots. Effects due to differentiation of the underlying basaltic magma are omitted.

deduced from the surface evidence by the Survey petrologists. Differentiation, however, has not been invoked. Harker's suggestion that the two magmas were complementary products of differentiation, a suggestion that has since been widely approved and adopted (e.g. *Ardnamurchan Memoir*, page 63) is thus found to be no longer without an adequate alternative in the circumstances here under discussion.

Returning to the basaltic magma in the cupola, it can now be

[1] A series of trial calculations based on various plausible relations between the forms and volumes of the basaltic and acid magmas in the completed cupola shows that ample heat is likely to be available to generate any reasonable amount of acid magma in a period of the order of a million years. For example, assuming that the cupola begins to fill with crystals from the base upwards at the same time as fusion of the sial roof begins, and allowing for latent heat and loss of heat by conduction for a million years, the volume of acid magma produced would be about a quarter of that of the basaltic part of the cupola.

shown that differentiation will inevitably take place as cooling sets in and the magma begins to crystallize, but that its course is towards the generation of a residual magma not of granitic, but of trachytic composition. The primary condition for the liberation of free silica, the formation of interstitial micropegmatite, or the separation of a residual acid magma is that the excess of olivine that crystallizes before the pyroxene stage should fail to react completely with the magma on further cooling. If magma and olivine crystals be separated before the reaction is completed then the former will necessarily carry an excess of silica. If, however, the olivine crystals remain in long continued contact with the slowly cooling magma, the potentially free silica will be used up again in the commutation of the excess olivine to pyroxene or in an equivalent reaction. In this case, as Bowen has clearly pointed out, (*Evolution of Igneous Rocks*, p. 236 et seq.) a trachytic differentiate would be determined. He also suggests that " very rapid cooling, such that the liquid was cooled through the temperature range, in which the excess olivine separates might permit a like result ". As being in accordance with the deduction he notes that the plateau-basalt magma of Mull when rapidly cooled develops analcite in its final residuum. Rapid cooling need not be considered in dealing with a cupola reservoir, but the example cited illustrates the principle that an originally undersilicated basaltic magma will still be undersilicated in the closing stages of crystallization unless an excess of olivine has separated and failed to react.

Inside the cooling cupola the temperature of the " excess-olivine "-pyroxene reaction will be reached first at the bottom and along the immediate neighbourhood of the walls. Consider the final critical stage when olivine can crystallize high up near the side at a temperature just above that of the reaction point. As each crystal sinks in (and with) the downward convection current, it will pass through a depth that corresponds to the reaction temperature at that pressure, and there will consequently be ample opportunity for the complete commutation of the excess olivine, with simultaneous crystallization of say, enstatite before the bottom is reached. At this stage, the bottom temperature will (for that depth and pressure) be well below the lowest temperature at which olivine could continue to crystallize. The subsequent ascending magma near the middle of the cupola will then have left behind it a concentration of crystals in depth, but it will have no excess of silica. This effect, which is like that brought about by slow cooling combined with stirring, depends on the difference between the real temperature gradient, maintained by convection, and the melting point gradient due to pressure (Fig. 2). As we have seen, its operation is fatal to the possibility of deriving a granitic differentiate from a cooling basaltic magma in a cupola. Instead, the abstraction from the magma of olivine, pyroxenes, calcic plagioclase, and iron-ores will lead to the concentration in the

residual magma of the constituents of the alkali-felspars and particularly of orthoclase, together with the volatile or " fugitive " constituents. The upper part of the magma should therefore pass through a trachybasaltic or mugearitic composition to a trachytic composition, provided that sufficient time is available. Daly has noted that trachytic eruptions are preceded by long periods of dormancy and heralded by great explosions due to the sudden escape of accumulated volatiles (15).

It may happen, if the heat supply is sufficient, that trachyte magma will be differentiating from the basaltic magma in the cupola while acid magma is still being generated at higher levels. This possibility arises from the fact that the fusion range of granite is well below that of basalt. Consequently, even while the basaltic magma is crystallizing there will be heat available to supply the necessary latent heat of fusion to the overlying sial roof. Such heat may finally have to pass through the trachytic magma, and though the convective circulation will then be far less simple than the ideal scheme portrayed in Fig. 4, the ultimate effect will continue to be the upward transport of excess heat. Certainly when this stage is reached the amount of excess heat remaining will necessarily be small, and one would normally expect most of the head of acid magma to be formed before the basaltic magma had developed a trachytic capping. Between the acid and basic magmas there would necessarily be a certain amount of admixture, and towards the end of the process the upper part of the cupola would be capable of providing such rocks as rhyolite, trachyte, and quartz-basalt, and various hybridized types. Here it may also be noted that while heated quartz crystals temporarily liberated from the sial roof (by the growth of a network of fluid between adjacent crystals) would not sink if they encountered basaltic magma, they would readily do so if underlain by successive layers of acid and trachytic magmas (16). It is in accordance with this deduction that Lacroix has found quartz inclusions in trachytic rocks and has recorded several occurrences of their transformation to tridymite or cristobalite (17).

The actual surface events at the initiation of each of the Ardnamurchan centres correspond very faithfully with the picture of cupola development that has now been roughly outlined. Of the volcanic vents of Centre I, the agglomerates of the Northern Vents contain fragments of trachytic and acid volcanic rocks ; while those of the Ben Hiant Vents " consist for the most part of angular pieces of trachyte, rhyolite, and other acid rocks, often vesicular ". Here, moreover, pitchstone lavas were poured out at intervals. The agglomerates were followed by a large intrusion of non-porphyritic quartz-dolerite and a plug or crater-infilling of porphyritic dolerite (*Memoir*, p. 73). Centre 2 was begun with doming up of the country rocks and the intrusion of cone sheets, some of which have " basic marginal portions and acid centres " (*Memoir*, p. 74).

Centre 3 consists almost entirely of ring-dykes. The outer and older ring-dykes are naturally more basic than the inner examples, since they would tap greater depths. The inner and younger ring dykes, fed from the higher levels of the cupola, include granophyre, various biotite-bearing rocks, and hybrid types described as tonalite and quartz-monzonite (*Memoir*, pp. 75 and 76). Of these last two it is stated on p. 99 that the evidence "strongly suggests that the various members of the Tonalite and Quartz-Monzonite Series are due to magmatic admixture or hybridization before intrusion".

Turning to Mull we find that mugearites occur with the plateau-basalts; that acid tuffs are associated with the basic lavas of the South-east Caldera; that the dyke-like channels initiated by the caldera fractures are occupied by large granophyre intrusions; and that the earliest cone sheets of the first centre are of granophyric and other acid and intermediate types. The enormous amount of acid material intruded during the first cycle seems to have temporarily exhausted the supply of granitic magma. The second cycle begins with basic cone-sheets, but as time went on more granitic magma was generated and the cycle concludes with acid intrusions, including the famous felsitic ring-dyke of Loch Bà.

The processes that have been suggested for the convective scooping out of a basaltic cupola and the subsequent differentiation towards trachyte are not without a bearing on the vexed question of the origin of felspathoidal rocks. Bowen has made the statement that "in the typical basalt-trachyte association the differentiation may proceed still further in the alkaline direction and give phonolitic trachyte with a deficiency of SiO_2 below that necessary to convert all the alkalis and alumina into feldspars" (*The Evolution of Igneous Rocks*, p. 239). The level of under-saturation in silica at which differentiation may proceed will depend partly on the composition of the original magma layer or basin or stream on which the cupola arises, and partly on the composition of the crustal layer in which the cupola forms. The cupola magma will be derived both from the fusion of the crustal layer and from the original magmatic source in depth. The composition of the latter may theoretically lie anywhere within the range from peridotite to basalt. We know very little of those peridotites that are not due to crystal accumulations from basic magma, but there are certainly alkali-peridotites such as kimberlite which appear to be independent of a basaltic origin, and in all such cases the degree of under-saturation in silica is extraordinarily low. I suggested several years ago that the initiation of igneous activity by deep-seated peridotitic magmas might lead to the production of a residual magma "relatively impoverished in silica, but enriched in alkalies and perhaps in a new series of volatile fluxes" (18). It now seems clear that the cupola mechanism described in this paper is capable of producing alkali-

residual magmas of just the type required to account for the alkali-rocks provided that it starts from a basal layer of alkali-peridotite.[1] Neither leucitite nor nephelinite, with or without olivine, can by any known possibility be differentiated from basaltic magma, but the former could easily be generated by crystallization differentiation from a kimberlite-like magma, and the latter from a corresponding magma with a higher Na/K ratio. Admixtures of alkali-peridotite and basalt magmas in various proportions would equally readily make possible such derived rocks as leucite- and nepheline-basanites, tephrites, and phonolites. I hope in a forthcoming paper with Dr. H. F. Harwood to develop this view in the light of specific petrological and chemical evidence.

The present paper, however, is mainly concerned with the origin of those acid rocks that are closely associated with basic rocks. The hypothesis of refusion of pre-existing crustal materials actuated by heat transferred from a lower level seems competent also to explain the long puzzling acid-basic association found in the lopoliths of Sudbury and Duluth, and the related but much less simple complex of the Bushveld. The Sudbury lopolith, for example, would be interpreted as due to successive intrusions of acid and basic magma, exactly as suggested by Harker (19), but instead of referring the two magmas to differentiation from a single parent stock occupying a deeper reservoir, the acid magma would be regarded as a product of refusion of the sial walls and roof of such a reservoir, the necessary heat coming from or through the underlying basic magma. The principles involved are the same as for the Tertiary central complexes. Only the scale and the form of intrusion are different. The transitional zone between the granophyre and norite of this great intrusion is noted by Harker as having " all the characteristics of a hybrid rock ".

In a recent study of the Sudbury lopolith it is stated that " there can be no doubt that the eruptive sheet is a unit formed by the segregation of a magma into a more basic portion, norite, passing without a break upwards into a more acid phase, micropegmatite " (20). On the contrary, there can be and is a very serious doubt. It seems most unlikely that this hypothesis—which is a return to the old view of differentiation *in situ*—can be sustained. The average composition of the alleged parent magma, taking the norite as one-third and the granophyre as two-thirds of the whole, differs rather widely from any common average igneous type, but it falls nearest to Daly's average quartz-monzonite. There is no evidence, so far as I know, that where quartz-monzonite

[1] Alkali-peridotite may readily be derived from average peridotite by subtracting eclogite and dunite from the latter. If the "subtract" be rich in eclogite, then the residual magma becomes enriched in potash, as in kimberlite. Early crystallization of enstatite lowers the silica percentage of the residual magma and may thus lead to the formation of melilite-basalt. Dominant separation of olivine favours the production of basaltic magma.

magmas have actually occurred they have split up into granophyre and norite. Nor are the renowned nickel-copper sulphide ores of Sudbury of a type that can be regarded as a probable associate of quartz-monzonites; they are, on the other hand, highly characteristic of norites and related basic rocks elsewhere. Harker's hypothesis of differentiation before intrusion has not to meet these grave difficulties, for the parent magma could then quite reasonably have been basaltic. Average basaltic magma under appropriate conditions could theoretically differentiate so as to produce about 10 per cent of granophyre. The intrusion of the latter, together with 5 per cent of the basaltic material, would then give the exposed proportions in the lopolith.

The object of this paper, however, has been to try and demonstrate that convection tends to prevent such differentiation, and that in the refusion of the sial crust an alternative and valid source for the acid magma that constitutes the problem is independently available.

In conclusion, I wish to thank my colleagues at Armstrong College, Mr. G. S. Mockler and Mr. S. Tomkeieff, for critically reading the first draft of this paper and discussing with me the various lines of evidence on which its conclusion depends. I also wish to take this opportunity to record my indebtedness to my friend Dr. A. Brammall, not only for his constructive criticism of this paper but also for the many inspired suggestions bearing on petrological problems that he has contributed to our discussions during the last ten years.

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The Heavy Mineral Correlation of Intrusive Igneous Rocks.

By A. Kingsley Wells, D.Sc., F.G.S., King's College, London.

OVER a period of some three years Dr. A. W. Groves has been working on the correlation of intrusions by their heavy mineral content, and the results have been published in three papers which have appeared in recent volumes of the Geological Magazine. The three papers read seriatim provide an interesting study of the evolution of an hypothesis which, if substantiated, would remove much, and in certain favourable cases, all of the uncertainty that usually shrouds the age of an intrusive rock-body.

In 1927 Dr. Groves,[1] by studying the heavy minerals separated from crushed specimens of some of the Jersey granites, attempted to judge between what may be termed the orthodox view (that these intrusives are of Pre-Cambrian age),[2] and that of the present writer (that, occurring as they do in a detached portion of Armorica, they may well be of Armorican age).[3] It was claimed by Dr. Groves that, if the Jersey and Armorican granites of South-West England are of the same age "... we should expect a strong resemblance in the heavy mineral suites, but no such resemblances are to be found; on the contrary, there are great differences". Hence, on the evidence collected, it was concluded that the Jersey granites are not Armorican, and they were correlated with intrusions lying unconformably beneath the presumed basal Cambrian conglomerates in Alderney.

After an interval of three years and in his latest paper on the subject,[4] Dr. Groves completely reverses his decision, and, although the Jersey granites still yield the same heavy mineral suites, he now finds sufficient points of similarity to justify the correlation of

[1] Geol. Mag., LXIV, 1927, 241–51.
[2] See, for example, G. H. Plymen, *Proc. Geol. Assoc.*, xxxiii, 1921, 163.
[3] In *Text Book of Petrology*, vol. i, The Igneous Rocks, 1926, 433.
[4] Geol. Mag., LXVII, 1930, 218–40.

these granites with those of North Brittany and the Cotentin. As one of the latter group has been shown on stratigraphical evidence to be probably of Permo-Carboniferous age, the Jersey granites are assigned to the same period, and doubt is thrown upon the stratigraphical evidence in Alderney, mentioned above.

In the interim I have examined the principles involved in the heavy mineral correlation of igneous rocks in a contribution, hitherto unpublished, to a discussion on the Evolution of Igneous Rocks; they were also made the subject of a paper [1] read before the British Association, Section C, at the meeting in Capetown, 1929. The potency of some of the objections to the wide application of this method of correlation, voiced on these occasions, has been appreciated by Dr. Groves; while others have been replied to in his latest paper. Certain obscurities and differences of opinion still remain, however, and there is some justification for publishing the case against the new principle, as presented to the Geologists' Association in December, 1928, together with further criticism arising from subsequent papers.

Problems connected with the ages of igneous rocks would be readily capable of solution were it possible to demonstrate that intrusions belonging to any one period of igneous activity share distinctive characters, essentially and exclusively their own. It is well known that even after long study, there is still room for much difference of opinion, even among experts, as to the age of certain igneous rocks in this country: the granites of Mt. Sorrel, the Channel Islands, and Skiddaw are notorious in this respect. That one writer should assign the last-named to the Ordovician, while others are convinced that it is Caledonian or Devonian, serves to emphasize the point.

One line of evidence that in certain cases is important is petrographic character. Recently Drs. Holmes and Harwood have shown that the basic dykes of the north of England fall into two regional groups. The one series, comprising the east-to-west dykes converge towards the well-known Mull swarm in the Hebrides, and thus the mere facts of distribution and trend are sufficient to establish their age as Tertiary—there is scarcely need for confirmatory evidence; but it is significant that these east-west dykes are essentially tholeiites; while the members of the second series, trending in another direction, are quartz-dolerites of the Whin Sill type. Thus, in this area, the occurrence of those petrographic characters implicit in the term "tholeiite" is confirmatory evidence of the Tertiary age of a dyke-rock, trending in a certain direction. Again, the hornblende-diorite-porphyries of Merionethshire are quite distinct from the dolerites and "diabases" of the area, and are clearly connected with the Rhobell Fawr eruptives of early Arenig age. Such cases are rare, however, and petrographic distinctiveness

[1] "On the Significance of the Accessory Minerals in Igneous Rocks": title only, in *Report of the Brit. Assoc.*, South African Meeting, 1930.

remains a desideratum that is diligently sought after, but rarely found. The idea that such distinctiveness is real carries with it the implication of magmatic uniformity during the periods of eruptivity. Without such magmatic uniformity, petrographic similarity would be unattainable. This carries us back ultimately to the idea that magmas, uniform during any one period, have varied in composition from age to age, sufficiently to impart a recognizable degree of petrographic distinctiveness to the rocks of the different eruptive periods. To all intents and purposes this is magmatic evolution, but whether or not we take this additional step, it will be readily admitted that, to put the idea to the test, we should examine the composition, on as wide a basis as possible, of the *basalts* of the several igneous cycles: these alone, of all the multiplicity of igneous rocks, may be trusted to represent average samples of the simatic substratum, and can be guaranteed as at least relatively free from the modifications of composition necessarily introduced during the slow uprise of more acid, more viscous magmas. So far as I know, such a comparison has not been made for this purpose, but opinion is moving in the right direction as is shown by the recent attempts of the Survey officers to establish "magma-types", as for example in the Tertiary rocks of Mull. However, it is my opinion that, were such an investigation undertaken, it would prove that the change from period to period has been of the same order as that introduced among the rocks of any one period by the "accidents" to which *all* magma-bodies are liable, and by the variation that is bound to come with differences in cooling history, as is more fully shown below for the case of the acid rocks. The granites stand on a different footing, and the question of the propriety of using petrographic characters as a criterion of age is necessarily inseparable from the even more difficult problem of the origin of granites. Within the last decade it has been shown that granite originates in bulk as a normal differentiate of a basaltic magma; that locally it may arise as a consequence of refusion of the sial; that sometimes it represents the light residuum left after the heavy, first-formed constituents have sunk away under the influence of gravity; and that locally it is produced by intense metamorphism and assimilation of siliceous sediments by basic magma. Although admittedly some of these cases are rare, it will be agreed that similarity in mineral content and chemical composition between two rocks that happen to fall in the same general category, but which have been produced by different genetic processes, can only be accidental, and wholly without age-significance. We must concentrate, therefore, on granites that have experienced the same pre-consolidation history, and the problem then resolves itself into deciding whether the individuality (capable of finding expression in terms of mineral-assemblage) impressed upon the magma of a certain period, could survive the act of uprise and the process of cooling. Before examining this possibility in detail,

a further restriction must be insisted upon. Harker's hypotheses on the effects of pressure upon a consolidating magma are too well known and appreciated to need recapitulation here. It will be sufficient to recall that the main effect is to give a kind of zonal arrangement of rock-types, the zones being parallel to the earth waves that are developing at the time, the rocks in different zones differing in several particulars, notably in alkalinity and flux-content, and the consequences thereof. The process does definitely operate on the small scale of an individual intrusion—our own geological record contains numbers of illustrations—and given the necessary magmatic mobility that Harker's hypothesis calls for, it follows that an Armorican granite in the Channel Islands (if it exists) would obviously differ in composition from one of the same age in Devon and Cornwall. The contemporary fold-axes trend roughly east-to-west, and it follows that Armorican granites occurring in the same latitude may reasonably be expected to show a close original degree of resemblance: those in different latitudes will differ to an extent determined by their separation measured across the folds. This essential principle was not realized by Dr. Groves when, in his first paper, he emphasized the absence from the Channel Isles of the most distinctive minerals of the south of England granites: the tourmaline, white mica, kaolin, topaz and others indicative of an unusual richness in fluoboric flux, and concluded therefore that these two sets of granites must be of different ages. The realization of the importance of this principle has presumably been the chief reason for the changed conclusions stated in his third paper.

Now, comparing rocks of the same petrographic type, of the same genetic history, and occurring in comparable situations in relation to the contemporaneous fold-axes, we shall expect identity of mineral composition if, and only if, the rock bodies have had the same cooling history: in other words, if they are alike in size, manner of injection, thickness of roof, etc. Otherwise some measure of heteromorphism will inevitably result. The only reasonable way of discounting the complications introduced by the operation of heteromorphism is to fall back upon bulk chemical analysis, i.e. to apply to the granites the method that the Survey officers are using for the basic rocks. In the case of the Dartmoor granite this has been done; but there is no other granite in the world which has been so much analysed and the chemical composition of which is so completely known. The other modern tendency, however, and the one that we are more immediately concerned with, is to ignore the bulk composition and indeed the essential constituents of the rocks and to concentrate upon the relatively small content of accessory minerals.

For the purpose of the present argument it will suffice to divide the accessory minerals in igneous rocks into several categories according to their genesis, as under:—

(*a*) *Normal Accessories.*—Those developed independently (so far

as is known), of any special controlling factors such as high flux content. In the acid rocks this includes the ubiquitous zircon, magnetite, ilmenite, some of the sphene, and the rarer xenotime, monazite, and perhaps orthite.

(*b*) *Pneumatolytic Accessories*, including tourmaline, fluorite, topaz, cassiterite, some of the rutile, anatase, and brookite, have been produced at a late stage in the cooling of the magma generally, although as Brammall has shown, the requisite flux-concentration has occasionally been attained quite early. It is generally conceded that a concentration of fluxes may be expected close up under the roof of any intrusion. It follows that the minerals listed above will be most abundant in the higher parts of an intrusion: both the kinds and proportions of the pneumatolytic minerals will vary as unroofing and denudation proceeds. They must be restricted to an upper, enriched zone of thickness not known; but the ultimate fate of this is complete removal by denudation. Therefore, even within the narrow limits laid down above (p. 258), the minerals of this class are without value as indicators of age.

(*c*) *Contamination Accessories* are those introduced into the magma from wall- or roof-rock. Without entering at all into the disputed question as to the extent to which the original composition of a magma may be modified by this process, we may insist upon the obvious fact that the visible products of contamination must be concentrated in the upper parts of the magma body: it is only here that the local excesses necessary to produce such minerals as cordierite, andalusite, sillimanite, spinel and garnet are likely to exist. In the deeper parts more completely established equilibrium will have smoothed out these excesses, and the mineral content will be normal. The contamination products are completely lacking in time significance.

(*d*) *Secondary Accessories* are those formed at the expense of the primary minerals by paulopost or subsequent changes. The commonest are the members of the chlorite, serpentine, and epidote groups. The development of these seems to be due to a richness of magmatic water, aided perhaps by pressure operating during the final stages of cooling. In the basic rocks of the Lizard Complex, where all stages between hornblende-schist and normal fresh gabbro occur within the limits of the one rock body, these minerals vary in proportion in a striking manner. Why should more significance be attached to their occurrence or non-occurrence in granites? Yet in his first paper (p. 246) Dr. Groves heads the list of differences between the "Older" and "Newer" granites of Jersey with "Absence of clinozoisite, zoisite, epidote, and hornblende" from the former, and on this and similar evidence claims to have confirmed the existence of granites of two ages in Jersey.

We are thrown back, therefore, upon the group of normal accessories, and have to rely upon such minerals as zircon that are common to all granites of all ages. This is now partly realized

by Dr. Groves, for in his third paper (1930) the hypothesis is stated thus: "Neighbouring granites, if comagmatic, should possess mineral suites with many points in common," and lower down, "that attention should be focussed on the primary stable minerals such as zircon, apatite, monazite, rutile, sphene, muscovite, biotite, hornblende, iron ores, etc." Elsewhere it is admitted, however, that in particular cases hornblende, biotite and sphene may be in large measure due to assimilation. They must therefore be removed from the category of primary stable minerals, as it is quite impossible to say what proportion of these minerals in any rock is due to contamination and how much is not. To fix an arbitrary "index figure" as Dr. Groves has done, is merely begging the question. The Cape Granite in South Africa contains rafts of roof-rock, Malmesbury Slate, that by intense metamorphism and probably transfusion have been converted into a relatively coarse-textured aggregate of quartz, biotite, and felspar, that in the hand-specimen is indistinguishable from a banded granite, while it is obvious that the biotite content varies with the degree of assimilation. The development of biotite, hornblende and sphene in granite which has assimilated gabbro has long been known from the work of Harker and Sollas in Skye and Barnavave respectively; while the writer and Dr. S. W. Wooldridge have described similar phenomena in the Jersey contaminated rocks. Similarly Grantham has shown that the hornblende-content of the Shap adamellite has been considerably increased by the assimilation of Borrowdale andesite wall-rock. Much biotite or little signifies nothing in the dating of an intrusion, for its amount depends upon reciprocal reaction. In heavy mineral correlation an abundance of biotite means only that the specimen came from the site of a more or less completely assimilated xenolith. Does not the "great paucity of biotite", which is the second of the chief differences between the "Older" and "Newer" Granites of Jersey, mean merely that the former is a purer granite than the latter?

Although placed in Dr. Groves' primary, stable minerals muscovite is, in many rocks, secondary after felspar, and indicates the functioning of magmatic waters at a late stage of crystallization.[1]

[1] It is indeed difficult to reconcile the statements concerning the stoping of granite into shale at l'Étacq, Jersey, with what is at present known of this process, in other localities. The sequence of rock types mentioned, viz., normal granite→muscovite granite→muscovite-pegmatite, cannot be admitted as a series showing progressive contamination; rather it seems clear that the magma was *not* contaminated with the shale, but had reached its present position too late to react with the country rock. The sequence illustrates the influence upon a cooling granite magma of an increasing concentration of flux, giving an abnormally high muscovite content, and an unduly coarse texture. . . . Although andalusite, etc., are absent from the specimens supplied to Dr. Groves (which is itself evidence of the relative purity of the rock), my own specimens show that this mineral does occur in xenoliths more or less completely assimilated by the granite magma, as at Les Rouaux; and also frequently in the numerous aplite veins in the granite. In both cases the andalusite is a highly pleochroic type and quite unmistakable.

As is well known, much of the rutile in granites is pneumatolytic. Thus the list of " primary, stable minerals " is reduced for our present purpose to three, apart from iron ores, viz. zircon, apatite, and monazite. In the third paper (1930), heavy mineral correlation has to all intents and purposes resolved itself into a close investigation of the zircons and apatites. Using the published data, the Jersey granites resemble those of Devon and Cornwall in respect of their zircons, many being distinctly zoned, while others are clear and of the same general type in both areas. Thus it appears that, in this work, the zircons are of paramount importance, and if Dr. Grove's claims are substantiated, correlation becomes delightfully easy.

It must be understood that two grades of distinctiveness are implied by these claims: the first may be given generic rank, and the second, less comprehensive and more specialized, specific rank. It is generic distinctiveness (for want of a better term) that would allow Armorican zircons to be distinguished from those of other ages—Pre-Cambrian, Caledonian, and Tertiary—in this country; while close comparison would allow Dartmoor zircons to be recognized as distinct from those obtained from other Armorican sources, by virtue of specific differences. Now this is, to the writer at least, almost incomprehensible. Generic distinctiveness, in order to characterize *all* Armorican zircons, must have been impressed upon the magma before it was separated into a number of more or less completely isolated bodies cooling under different conditions, and could hardly have resulted from other than chemical causes. These would conceivably govern the quantity and colour of the zircons, and the nature of their inclusions. Within the limitations laid down by these chemical factors there is room for less fundamental, though possibly more obvious, variations, resulting from differences in cooling conditions following intrusion. By comparison with salts it is reasonable to assume that the factors involved are concentration, temperature gradient, thickness of roof (governing the pressure), and penetrability of the roof (governing the escape of fluxes). Thus, in theory at least, there is little difficulty in admitting that circumstances might combine to produce a unique type of zircon: it is the case with other minerals. Those with long experience in the South African diamond fields claim to be able to specify the particular mine from which a stone of (to them) unknown origin was obtained. Even with zircon, most mineralogists would have no difficulty in recognizing the ruby-red crystals from the Niedermendig lavas: they stand out as aristocrats from among a swarm of undistinguished, dun-coloured individuals. With regard to the Armorican zircons, however, I cannot see that they have much right to the claim of distinctiveness. Two factors are emphasized in the published accounts: their zoning and their habit. The zoning is a physical phenomenon, and is not exclusively an Armorican attribute. The habit is certainly not distinctive.

Brammall records from the Dartmoor granite alone eight varieties, all practically identical with forms figured in Dana's *Mineralogy* from different localities and of widely different ages. Judging from the published figures the dominant Armorican zircons show just the simple combination of tetragonal prism (110) and tetragonal bipyramid (111) in zone with the former. This combination is common the world over.[1]

In spite of these criticisms, however, one is reminded that the expert palaeontologist, by virtue of long study can, with uncanny vision, see distinctiveness in fragments of ill-preserved graptolites that are the despair of the inexpert. Are there equally subtle differences, obvious enough to the expert, but difficult to specify or even to demonstrate, which enable him to distinguish Dartmoor zircons from all others, however improbable it seems in the abstract?

Before Dr. Groves has proved his case it should be shown (1) in what particulars the Armorican zircons are unique; (2) that there is no appreciable difference in habit with increasing depth in an intrusion. If there is, and such appears to be the case, the "St. Austell type" (if it exists) in the surface layers, may well give place in depth to a "Dartmoor type".

Only in so far as the cooling history of an intrusion has been unique can one reasonably expect the rocks to yield a distinctive heavy mineral suite: only in as much as the cooling history of two or more intrusions has been alike can one expect similarity in heavy mineral content. To use heavy minerals for correlation is to claim both the uniqueness of the first, and the similarity of the second supposition. It is asking much, and in the writer's opinion, is not proven.

The correlation of sedimentary rocks by means of their accessory minerals is itself a difficult matter, and the method has obvious limitations. It does, nevertheless, depend upon a simple and comprehensive principle: a certain suite (determined by the rocks undergoing destruction at the time), will characterize a certain distributive province. No such clear and definite principle has yet been established as the basis of correlation of igneous intrusions by their accessories, and the method must remain inconclusive.

[1] The writer is strongly of the opinion that precise nomenclature is essential in describing these crystals; the terminal faces should be referred to as *bi*-pyramids (not pyramids, which belong to Class 9, the Hemimorphic Class); while distinction should be made between tetragonal bi-pyramids and ditetragonal bi-pyramids. For example, Dr. Groves speaks of "the simple combination of prism and unit pyramid (111). The steeper pyramid (311) is generally absent." From its context, one cannot help wondering whether this is a misprint for "steeper (bi)pyramid 331", a common zircon form; but so is the ditetragonal bi-pyramid 311, and so one is left guessing, as the drawings do not help.

On the Relations of the Chalky Boulder Clay to the Implementiferous Beds of the Pleistocene Formation.

By J. T. Banton, B.A., sometime Scholar of Pembroke College, Cambridge.

As is well known, there is a great diversity of opinion with regard to the relationship of the implement-bearing beds of the Pleistocene formations and the Chalky Boulder Clay. Any facts which throw light on this vexed question might, I thought, be very acceptable and useful and I am endeavouring in this paper to bring forward what evidence I can bearing on this very difficult and perplexing subject.

The River Gravels of the Oxford District.

Perhaps the latest and most important work bearing on this subject is the researches of Dr. K. S. Sandford in the Oxford district, and described in a paper in the *Q.J.G.S.*, vol. lxxx, part 2, pp. 113–179, with the above title. In it he describes what he calls the Plateau drift, 140 feet above the level of the Thames basin, consisting of a very heavy tenacious ferruginous clay full of quartzite boulder, and pebbles chiefly from the Bunter formation in the Midlands, some of them striated, and below that, four different river terraces, viz. :—

The highest, or Handborough Terrace . .	100	feet	above	the river level.
The Wolvercote Terrace, including the Wolvercote Channel	40	,,	,,	,,
The Summertown-Radley Terrace . .	20	,,	,,	,,
And lastly the Flood Plain Gravels . .	10	,,	,,	,,

A remarkable fact is that the terraces contain Bunter pebbles, evidently derived from the Plateau drift, and in the Wolvercote and Handborough terraces quartzite boulders similar to those contained in the Plateau drift much battered and showing ice striae, *vide* p. 122. With the exception of the Handborough terrace to which a doubtful find of a Chellean implement is attributed, all the terraces contain implements of man, ranging from Chellean to those of Micoque type, together with abundant mammalian remains, *Elephas, Rhinoceros, Hippopotamus,* etc.

Pleistocene Deposits at Foxhall Road, Ipswich.

With regard to these deposits it was thought that certain fluid implements were contemporaneous with the Boulder Clay, but subsequent investigations have somewhat modified this view.

In a paper by Mr. Reid Moir and Professor Boswell, in the *Journal of the Anthropological Institute*, liii, 1923, the following sections are given :

	Ft.	in.
1. Sandy surface soil	2	7
2. Fine reddish gravel, with pockets of sand	1	3
3. Strong reddish mottled sandy clay, etc.	3	0
4. Laminated brick-earth weathering yellow-brown, etc.	3	3
5. Bluish brick-earth, with scattered stones	3	0
6. Sandy do.	1	9
7. Gravelly and sandy loam	1	9
8. Sandy loam with a few signs of bedding	2	0
9. Gravelly sandy and clayey band		6
10. Mottled clayey loam, with much water		6
11. Sandy clay with chalk fragments	2	6
12. Shingly gravel	1	0
13. Reddish loamy sand bored for	5	9
14. Dark grey Boulder Clay, with many Chalk pebbles and derived Jurassic shells	2	6

Flint implements to the number of 544 were found in beds 2, 4, 5, 6, and 7, and chiefly of Achenlian and Mousterian types.

The outstanding feature of the Foxhall Road section is the occurrence of typical Chalky Boulder Clay at the *base* of the deposits and underlying the implement-bearing beds, *vide* p. 233, in which it is stated that these excavations go to prove that the material at the bottom of the boring is typical Chalky Boulder Clay, and farther on, p. 241, " The deposit at Foxhall Road shows none of the features just mentioned, which are characteristic of the North Sea drift, on the contrary it displays all those of the Chalky-Kimmeridgic Boulder Clay."

Implement-bearing Beds at Hoxne, Suffolk.

Here we appear to have a repetition of the same story as at Ipswich.

Important excavations have recently been made at this classical locality by Mr. Reid Moir, on behalf of the Committee of the British Association, particulars of which are given in a paper in the *Proceedings of the P.S.E.A.*, 1926, vol. v, part 2, pp. 144 to 148.

Several sections are given and in all these Kimmeridgic Boulder Clay is seen *underlying* implement-bearing beds. It is important to notice that the Boulder Clay is overlain by a Lacustrine bed, 20 feet thick, indicating a temperate climate and above this an Arctic bed, 20 feet thick, showing a cold period, and over this are the implement-bearing beds containing late Achenlean and Mousterian types, *vide* section p. 152.

Overlying all these is a deposit which is regarded as an upper Boulder Clay, but by some authorities this is what the late Mr. Osmond Fisher called Trail, and which is said to contain derived implements of different ages ranging from pre-Chellean to Mousterian. With regard to these implements, if I may venture to say so, it by no means follows that they were derived from deposits older than the Kimmeridgic Boulder Clay. Indeed the thickness of the beds intervening between the Kimmeridgic Boulder

Clay and the overlying implement-bearing beds, about 40 feet, would seem strongly to support this view as a long lapse of time is indicated, including two changes of climate, the aforesaid beds being non-implementiferous. I would also here remark that Mr. Reid Moir regards the Kimmeridgic Boulder Clay as the equivalent of the Cromer Till. In places, however, where the Kimmeridgic Boulder Clay is seen in juxtaposition with the Till, it always overlies the latter deposit and is quite distinct from it; e.g. in the cliffs at Corton, near Lowestoft, and at the brickyard at Somerleyton, near the same place.

Moreover, Dr. Sandford, in his paper previously referred to, states that all the terraces contain implements of man, ranging from Chellean to those of Micoque type.

For a list of further localities connected with this question, I would refer the reader to a paper by Mr. Hazzledine Warren, entitled "Pleistocene Classifications", in the *Proceedings of the Geologists' Association*, vol. xxxv, part 4, pp. 265–382.

The following is a selection:

Derwent valley—the pre-glacial valley is filled with Boulder Clay, through which the river has cut its present channel. Associated with this re-excavation there is a gravel terrace running for some miles south of Derby at a regular level of 15 and 20 feet above the present alluvium. A section of this gravel yielded a number of bones of *Hippopotamus*, which were not derivative, but well preserved and belonging to one animal. There were also twenty species of temperate plants.

The top of the gravel presented the usual phenomena of trailing referable to the Ponder's End cold stage, but the regular conformity of the gravel terrace to the alluvium, and to the present rivers, precludes the occurrence of any serious glacial episode since the re-excavation of the valley through the Boulder Clay.[1]

Essex, Blackwater Valley.—Boulder Clay is reported to occupy the valley in certain places and the Pleistocene River Gravel at Kelvedon and Witham is said to overlie this Boulder Clay.[2]

Kelvedon Tollesbury new Railway Sections.—Many years after the above record the Essex Field Club visited the excavations in progress, and Mr. Whitaker is reported to have found a palaeolithic flake in the river gravel. Chalky Boulder Clay was observed underlying the gravel.[3]

Felstead Stebbing District is also claimed to furnish similar evidence.[4]

Braintree.—Pleistocene brickearth with *Elephas* and palaeolithic

[1] H. H. Arnold Bemrose and R. M. Deeley, *Quart. Jour. Geol. Soc.*, lii, 1896, 497.

[2] W. H. Dalton, *Trans. and Proc. Essex Field Club*, ii, 1881, 15, and *Essex Nat.*, iv, 1890, 104.

[3] *Essex Nat.*, xiii, 1904, 249.

[4] J. French, *Essex Nat.*, vi, 1892, 132.

flakes flanks the Holocene alluvium of the valley. It seems incredible that the Glacial Period, with its Boulder Clays, etc., can come between them.[1]

Great Yeldham.—Brickearth and gravel with *Elephas*, *Rhinoceros*, etc., are stated to lie in a hollow in Boulder Clay unconnected with present streams.[2]

Reviewing the whole position, it only remains to say that if the theory is held that the Boulder Clay overlies and is newer than the implement-bearing beds of the River Drifts and Lacustrine deposits, it is inconceivable that they were not long ago swept away when we consider the enormous devastating agencies at work during that period—the immense thickness of the Boulder Clay in various localities being evidence of this. I have myself seen the Boulder Clay capping a hill in the valley of the Ouse which was being excavated for a reservoir, and in which a depth of 30 feet was reached without going through the Boulder Clay.

In conclusion, I beg to tender my best thanks to Dr. Sandford, Mr. Reid Moir, Mr. Hazzledine Warren, and Professor Marr for copies of their papers and for useful hints and information.

Note.—As regards the implements, I have not touched on the question of ice striae, as it is not possible to attach much importance to it. In the first place the material (flint) of which they are constructed is so hard that the impressions made would be too slight to enable one with any degree of certainty to decide whether they were the result of ice action or some other agency, such as "soil creep", etc., and also it would be rather difficult to tell if the scratches were due to an earlier or later glaciation. It is a significant fact that at Ipswich the greatest percentage of scratched flints occurs at the top of the Foxhall Road section. Since this paper was written, the researches of Mr. Leakey in E. Africa have brought to light the important fact that a race of man of a higher and lower grade of culture were in existence at the same time, i.e. the Mousterian and Aurignacian. This should modify our views considerably with regard to the history and origin of man.

A Further Note on Chlorophaeite and Palagonite.

By L. Leigh Fermor, O.B.E., D.Sc., A.R.S.M., Geological Survey of India.

I HAVE read with very great interest Dr. Peacock's paper on the distinction between palagonite and chlorophaeite in the Geological Magazine, for April, 1930. On p. 170 thereof, Dr. Peacock gives a brief summary of my conclusions upon the nomenclature of chlorophaeite and palagonite as given in my paper

[1] *Essex Nat.*, viii, 1894, 155, 219. E. T. Newton, *Essex Nat.*, ix, 1895, 16. T. V. Holmes, *Essex Nat.*, ix, 1895, 10.

[2] J. W. Kenworthy, *Essex Nat.*, xi, 1899, 96; and S. V. Holmes, *Essex Nat.*, ix, 1895, 19.

on the same subject.[1] Briefly, my conclusions are that as far as can be judged from existing analyses, the hydrous glassy material forming a portion of the rocks originally called palagonite has a composition that brings it within the range of composition exhibited by various specimens of chlorophaeite, and that, therefore, the term palagonite should be reserved for the impure rock, and the term chlorophaeite used for this hydrous glassy material. It was also recommended by me that the process by which in the case of the basaltic tuffs the primary glass becomes hydrated should be termed palagonitization and that the same term should be applied to the process by which the interstitial glass of basaltic lava flows is altered with production of chlorophaeite. The fact that olivine, if present, a portion of the augite, and occasionally some of the felspar, may also suffer similar alteration or be replaced, is not regarded by me as pointing to an essential difference in the two processes.

In this present paper Dr. Peacock sets out to show that the hydrous glassy material of palagonite is essentially different in composition from chlorophaeite. This is the chief point of the paper, for if there is no essential difference between the hydrous glassy material of palagonite and chlorophaeite, the same name should be given to both irrespective of origin. My opinion, after carefully reading Dr. Peacock's paper, is that he has failed to establish the point he set out to prove.

In Table I of his paper he gives a series of seven analyses of chlorophaeite arranged in order of increasing alumina, the amount of alumina ranging from 0 to 10·49 per cent. The first three analyses with alumina ranging from nil to 1·54 per cent represent, according to Dr. Peacock, the typical chlorophaeite of orange colour.[2] The last three containing from 5·40 per cent of alumina to 10·49 per cent represent, according to Dr. Peacock, green chlorophaeite.[3] Whereas the brown chlorophaeites contain from 21 to 26 per cent of ferric oxide and only 2 to 3 per cent of ferrous oxide, the green chlorophaeites contain each about 12 per cent of ferric oxide with ferrous oxide ranging from 1·6 to 14·6 per cent. From this Dr. Peacock points out that the materials called palagonite in basaltic lava flows comprise two phases of chlorophaeite, namely the brown phase and the green phase, of which the brown phase is high in ferric oxide and the green phase high in ferrous oxide and probably also in alumina. The defect in this classification is including the chlorophaeite of the Giants' Causeway with the green chlorophaeites. Although the chlorophaeite of the Giants' Causeway is high in alumina and is the lowest of the seven analyses in ferric oxide, it is also the

[1] *Rec. Geol. Surv. Ind.*, lx, 1928, 411–30.

[2] But it must be noted that a reference to Heddle's original paper (*Trans. Roy. Soc. Edinb.*, xxix, 86) shows that of the material selected for analysis, one-fifth was green in colour and none had changed further than to a rich asphalt-brown.

[3] But is must be noted that according to Heddle (*ibid.*, 88) the material used for analysis VII was a deep rich brown in colour.

lowest of the seven in ferrous oxide and is brown in colour. It may be suggested, therefore, that the proportion of alumina has nothing to do with the colour, and that the latter depends on the proportion of ferrous oxide with reference to the ferric oxide. I give below a passage by myself upon the difference between orange and green palagonite, and I am prepared to accept, for the purposes of this discussion, Dr. Peacock's view that green palagonite, instead of being delessite as therein suggested, is a green phase of chlorophaeite. The quotation [1] is as follows :—

"Palagonite in General.

"From the foregoing notes we see that under palagonite have been comprised at least two distinct substances. Treated generally, palagonite may perhaps be regarded as a hydrous glassy substance of variable composition formed partly by hydration of the primary glass and partly at the expense of augite (? again largely hydration) and iron-ore, and with much more difficulty at the expense of felspar (? by replacement). In colour it ranges through orange and brown to brownish green and bright green, and the material of all colours may become 'devitrified' and anisotropic in spherulites and concentric-radiate layers, with in all cases a positive elongation to the fibres. The clear orange and brown varieties, both isotropic and anisotropic, constitute chlorophaeite, whilst the anisotropic green form is probably identical with delessite or celadonite. It is exceedingly difficult to distinguish these two latter minerals one from another when in fine aggregates, but if one may assume the green mineral of the palagonite to be delessite, then the difference between orange palagonite (chlorophaeite) and green palagonite (delessite) is very simple.

"Chemical Difference between Orange Palagonite (Chlorophaeite) and Green Palagonite (Delessite).

"A reference to the analysis given on p. 136 will show at once that in most respects these two minerals are chemically very closely allied; the chief difference is that in chlorophaeite the iron is mainly in the ferric condition whilst in delessite it is mainly in the ferrous condition In addition, whilst delessite contains alumina as an essential constituent, the chlorophaeite of the original locality is practically devoid of alumina. The chlorophaeite of Giants' Causeway, however, yielded 10 per cent of Al_2O_3. Finally, chlorophaeite is more highly hydrous. It appears, therefore, legitimate to deduce that the formation of one or other of these two minerals is largely a function of degree of oxidation at the time of alteration. As both delessite (30 per cent) and chlorophaeite (36 per cent) contain a considerably smaller percentage of silica than any of the original substances, primary glass (about 50 per cent), pyroxene (about 55 per cent), and labradorite (about 55 per cent)—except iron-ore, their formation as a secondary product involves the separation of silica. This may, of course, be either removed in solution to be deposited in geodes and vesicles, or it might crystallize out as an additional complementary mineral with the palagonite. In the case of the green palagonite, where Al_2O_3 is retained, the surplus silica has often appeared as the enclosed spherulites of chalcedony noticed above. But in the case of chlorophaeite, as the Al_2O_3 of the original minerals is not always retained, the destination of this constituent appears also to require explanation. The chabazite supplies the obvious answer, for it explains the destination of both surplus Al_2O_3 and SiO_2, which may be assumed to have taken over such alkalies as were available. From this it should follow that chlorophaeite is lower in alkalies than delessite. Alkalies

[1] "On the Basaltic Lavas penetrated by the Deep Boring for Coal at Bhusawal, Bombay Presidency": *Rec. Geol. Surv. Ind.*, lviii, 1926, 133.

cannot, however, be regarded as an essential constituent of either mineral, and the published analyses suggest no marked difference in this respect. If, therefore, the green mineral be delessite it seems necessary to assume that its formation has involved the removal of alkalies in solution. Both chlorophaeite and delessite are low in lime ; in the former case the destination of this constituent, at least in part, is the accompanying chabazite, but in the formation of delessite lime must also have been removed in solution."

It is unfortunate that whereas it has been possible to pick out pure specimens of chlorophaeite for analysis, for example my specimen II of Dr. Peacock's Table I, it has not been possible to pick out equally pure material from the palagonitic rocks of Sicily and Iceland. Dr. Peacock is able, however, to give two analyses in his Table II. One is an analysis of a palagonite coating a fragment of basic volcanic glass collected from the depths of the Pacific Ocean by the *Challenger* expedition. This analysis is unsatisfactory because it was made on a sample after drying at 105° C., so that it is necessary to guess the amount of water driven off below 105° C. The second analysis of palagonite given in Table II has been deduced from an analysis of palagonitic rock containing 76 per cent of the glassy substance itself. This is the same analysis as the one from which I calculated the composition of palagonite given in the Table on p. 415 of my paper in *Records, Geological Survey of India,* lx. The difficulty of basing reliable conclusions upon an analysis so calculated will be realized if one compares the different results obtained by Dr. Peacock and by myself in deducing the composition of the palagonite itself from the analysis of the palagonitic rock. The most substantial difference is that whereas I show 7·5 per cent of magnesia, Dr. Peacock shows only 2·3 per cent.

Dr. Peacock then compares the analyses of palagonite given in his Table II with the analyses of chlorophaeite give in his Table I, and thus appears to show that there is an essential chemical difference between palagonite and chlorophaeite, namely that palagonite is little more than half as rich as chlorophaeite in the ferro-magnesian oxides and more than twice as rich in alumina. This is done by comparing an average based on seven analyses of chlorophaeite with an average based on two analyses of palagonite. These two analyses of palagonite are, however, highly aluminous, and, therefore, should be compared only with the aluminous varieties of chlorophaeite. This I do below in Table I, comparing with the analysis of the aluminous chlorophaeite of the Giants' Causeway both my version and Dr. Peacock's version of the composition of the Iceland palagonite as deduced from the analysis of the palagonitic rock, and the analysis of the palagonite from the Pacific. On comparing these analyses it will be seen that the only substantial difference between the analysis of the palagonite of Iceland as calculated by Dr. Peacock and that of the chlorophaeite from the Giants' Causeway is in the magnesia, of which the former analysis shows 2·3 per cent and the latter 10·52 per cent. If, however, my version of the composition of the Iceland palagonite be used, then

the difference in the magnesia contents becomes of little importance, namely 7·5 against 10·52 per cent. If one compares the composition of the Pacific palagonite with that of the Iceland palagonite, it is seen that there is as substantial a difference between the composition of these two palagonites as there is between the composition of the Iceland palagonite and the chlorophaeite from the Giants' Causeway; for whereas the Pacific palagonite contains only 2·5 per cent of lime, the Iceland palagonite contains 7 or 8 per cent of lime. On the other hand, whereas the Pacific palagonite contains 7·2 per cent of alkalies, the Iceland palagonite contains at the most 0·3 per cent. Below the analyses in Table I are shown the sums of ferro-magnesian oxides, of the sesqui-oxides, and of the protoxides and alkalies. It will be seen that the only substantial difference in any of these totals is in the protoxides, which amount to only 6 per cent in the Pacific palagonite against 17 per cent in the chlorophaeite of the Giants' Causeway. The Iceland palagonite, however, shows 12·2 and 14·5 per cent of protoxides, and is, therefore, nearer to the chlorophaeite of the Giants' Causeway than to the Pacific palagonite. If one adds the alkalies to the protoxides, however, as I do in my formula for the chlorophaeite series, then the difference between Pacific palagonite and the Iceland palagonite in this respect disappears.

TABLE I. ANALYSES OF ALUMINOUS CHLOROPHAEITE AND PALAGONITE.

	Giants' Causeway.	Iceland.	Iceland.	Pacific.
	I.	II.	III.	IV.
SiO_2 . .	35·99	35	33·2	37·8
TiO_2 . .	—	2·5	2·8	—
Al_2O_3 . .	10·49	8	9·6	13·7
Fe_2O_3 . .	11·89	13	13·3	12·3
FeO . .	1·63	0	1·6	—
MnO . .	0·08	—	0·3	2·5
MgO . .	10·52	7·5	2·3	1·9
CaO . .	5·15	7	8·0	1·6
Na_2O . .	0·76	—	0·0	3·8
K_2O . .	0·34	—	0·3	3·4
H_2O+ . .	9·04	27	7·6	8·0
H_2O- . .	14·16		20·6	15·0
Total . .	100·05	199·6	100·0	100·0
$Fe_2O_3+FeO+MnO+MgO$	24·12	20·5	17·5	16·7
R_2O_3 . .	22·38	21	22·9	26·0
RO . .	17·38	14·5	12·2	6·0
$RO+R_2O$.	18·48	14·5	12·5	13·2

I. Analyst M. F. Heddle.
II. Calculated by me from analysis by W. H. and F. Herdsman.
III. Calculated by Dr. Peacock from same analyses as II.
IV. Analyst Sipöcz.

It is not perhaps sufficient to show, in so far as the imperfect chemical data permit, that palagonite falls in composition within the limits of composition of chlorophaeite, in order to convince the reader that the palagonite of Iceland must be regarded as one form of chlorophaeite, for Dr. Peacock draws other distinctions besides the chemical ones. He points out that the palagonite has a resinous appearance, whereas chlorophaeite is pitch-like. Chlorophaeite is only pitch-like when the specimen has been exposed to air. The freshly-broken material is yellow-green to golden yellow.[1] In several cases it has been observed to change colour rapidly after exposure through dark shades of green to black. The following passage is worth quoting (loc. cit., p. 94) :—

> "On freshly fractured surfaces of the rock, this 'glass' is pale golden in colour, but it speedily changes colour through darker shades of green to black, and finally to ruby-red, and eventually it may suffer decomposition on into a limonitic powder. Many of these spherules when decomposed show a concentric shell-like structure."

In the passage quoted, Dr. Fox and I have mentioned that chlorophaeite may suffer decomposition into limonitic powder. Heddle also mentions a chocolate-brown decomposition product from the Isle of Rum. The colour of the decomposition product presumably varies with the amount of iron in the chlorophaeite, and, though there are no data on the subject, it is possible that the aluminous chlorophaeite of the Giants' Causeway may give as pale a decomposition product as the palagonite of Iceland.

Dr. Peacock, in his summary, states that "chlorophaeite occurs in doleritic rocks by the reaction of magmatic fluids with ferromagnesian minerals, most commonly olivine, and perhaps sometimes with ultra-basic vitreous residues". If this statement is based upon my papers, then he has misunderstood me. Thus "my own conclusion is that the major portion of the palagonite in the Bhusawal traps has been formed at the expense of the original glass, and in many slides the primary glass has completely disappeared and is now entirely represented by palagonite".[2] Palagonite is also formed at the expense of augite and iron-ore, whilst even plagioclase is sometimes replaced by palagonite. It must not be overlooked, however, that the major portion of what I have called palagonite has been formed at the expense of primary interstitial glass of the basalt.

Dr. Peacock also frequently refers to the rocks containing chlorophaeite as "doleritic rocks", whilst he uses the adjective "basaltic" when speaking of the tuffs of Iceland. He thus appears to be making another distinction. As far as India is concerned, the rocks containing the chlorophaeite are mainly basalts and, as a rule, it is only the central portions of very thick flows that are coarse enough to be

[1] See L. L. Fermor and C. S. Fox, *Rec. Geol. Surv. Ind.*, xlvii, 94–9.
[2] *Rec. Geol. Surv. Ind.*, lvii, 126.

called dolerites. In the list of specimens at the end of my paper upon the lavas of Bhusawal, of those rocks described as containing palagonite or chlorophaeite, twenty-seven are entered as basalts, and six as dolerites, whilst one is termed a "doleritic basalt". Further, in paragraph 6 of his summary, Dr. Peacock states that with certain minor changes palagonite has substantially the chemical composition of basalt to which 20 or 30 per cent of water has been added. Combining this with No. 2 of his conclusions, the implication is that chlorophaeite has not a composition substantially that of basalt. In order, therefore, to show the true state of affairs, I have put together in Table No. II, after recalculating to 100 per cent on a water-free basis, the average analysis of eleven Indian Deccan Traps (including one from Rajmahal), and analyses of the low-alumina chlorophaeite of Nagpur, of the high-alumina chlorophaeite of the Giants' Causeway, and of the palagonites from the Pacific Ocean and Iceland. A study of these figures shows that compared with basalt the Nagpur chlorophaeite has lost in alumina, lime, and alkalies, and gained in magnesia and total iron, which has mainly passed from the ferrous to the ferric condition. The chlorophaeite of the Giants' Causeway shows little change in alumina and total iron, though, as before, there has been a conversion of ferrous into ferric iron and an increase of magnesia and loss of lime and alkalies. The palagonite of Iceland, like the chlorophaeite of the Giants' Causeway, shows alumina and total iron similar to those of basalt, with a similar loss of alkalies, but some increase in lime. The direction of the change of magnesia is doubtful. It depends upon which calculated analysis is used. The palagonite of the Pacific Ocean shows high alumina, high alkalies with loss of lime and magnesia, and total iron about the same. In none of these cases does it appear that the resultant product can be described as basalt with only minor changes, except with the addition of water.

Discussing my general formula for chlorophaeite, Dr. Peacock remarks (loc. cit., p. 172) :—

> "Analyses of chlorophaeite and one indirect estimate of the composition of palagonite can, it is true, be approximately embraced by the formula proposed by Dr. Fermor. But this formula also forms the basis of a proposed 'chlorophaeite series' of minerals embracing, as Dr. Fermor shows, twenty other minerals, including such definite species as some zeolites and even biotite."

Dr. Peacock has, I am afraid, misunderstood me. A list of eighteen hydrated minerals including chlorophaeite and palagonite with the ratio $R_2O_3 : SiO_2 = 1 : 4$, is given,[1] as possible members of the chlorophaeite series. But it is shown that of the sixteen additional minerals fifteen have too little water to be included in the series, and the only one of these accepted as belonging to the chlorophaeite series is neotocite, which is regarded as a mangan-chlorophaeite. The palagonite of Iceland, whichever deduced analysis be used,

[1] *Rec. Geol. Surv. Ind.*, lx, 1928, 423.

TABLE II. ANALYSES OF BASALT, CHLOROPHAEITE, AND PALAGONITE REDUCED TO A WATER-FREE BASIS.

	I. Basalt.	II. Chloro-phaeite.	III. Chloro-phaeite.	IV. Pala-gonite.	V. Pala-gonite.	VI. Pala-gonite.
	Deccan Traps.	Nagpur.	Giants' Causeway.	Iceland.	Iceland.	Pacific Ocean.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
SiO_2 . .	51·85	51·71	46·83	46·5	47·9	49·09
TiO_2 . .	1·96	—	—	3·9	3·4	—
Al_2O_3 . .	13·92	1·47	13·65	13·5	11·0	17·79
Fe_2O_3 . .	3·27	32·02	15·47	18·6	17·8	15·97
FeO . .	10·17	3·21	2·12	2·3	—	—
MnO . .	0·17	0·51	0·11	0·4	—	3·25
MgO . .	5·59	7·39	13·69	3·2	9·6	2·47
CaO . .	9·68	3·69	6·70	11·2	10·3	2·08
Na_2O . .	2·66	—	0·99	—	—	4·93
K_2O . .	0·73	—	0·44	0·4	—	4·42
Total .	100·00	100·00	100·00	100·00	100·00	100·00
$Al_2O_3+Fe_2O_3$	17·19	33·49	29·12	32·1	28·8	33·76
Fe_2O_3+FeO.	13·44	35·23	17·59	20·9	17·8	15·97
Fe_2O_3+FeO+ MnO+MgO	19·20	43·13	31·39	24·5	27·4	21·69
RO . .	25·61	14·80	22·62	17·1	19·9	7·80
R_2O . .	3·39	—	1·43	0·4	—	9·35
$RO+R_2O$.	29·00	14·80	24·05	17·5	19·9	17·15

I. Washington's average of 11—including one from Rajmahal.
II. Analyst P. C. Roy.
III. Analyst M. F. Heddle.
IV. Calculated by Dr. Peacock from W. H. and F. Herdsman's analysis of palagonite rock.
V. Calculated by me from same analysis as IV.
VI. Analyst Sipöcz.

contains enough water for the chlorophaeite series ; it is not possible to test this point in the case of the analysis of the Pacific Ocean palagonite, for the water driven off below 105° C. was not determined.

My conclusion then is that the hydrated glassy substance in the palagonitic tuffs of Iceland has a composition that brings it chemically within the range of composition of chlorophaeite, this " palagonite " belonging to the aluminous end of the range. This palagonite also has a refractive index appropriate to its position in the chlorophaeite series (see fig. 1 of my paper, loc. cit., p. 419). The fact that the colour of the material from Iceland is not green, although chemically it is towards the aluminous end of the series where the chlorophaeite tends to be green, does not appear to me to be material, especially as it carries very similar proportions of ferric and ferrous oxides to the aluminous chlorophaeite of the Giants' Causeway which is described as deep rich brown in colour.

The fact that the alteration of the basaltic glass of Iceland is supposed to have taken place at a lower temperature than it is thought prevailed when the interstitial glass of the basalts of India was altered does not appear to me to introduce an essential difference, even though in the latter case there was in addition some alteration, if necessary replacement, of definite minerals, e.g. olivine (if present), augite and felspar (occasional).

I find myself driven back to my previous conclusion, namely that palagonite as originally described is a rock containing a secondary hydrous glassy substance (a mineraloid), which on the meagre chemical evidence at present available must be accepted as having a chemical composition within the range of composition of the secondary hydrous glassy substance or mineraloid to which the term chlorophaeite has been applied. For this reason it has appeared to me unnecessary to abandon the use of the term palagonite to describe the impure secondary products found in plateau basalts in several countries and the use of the term palagonitization to describe the process by which this has taken place, whether the temperature of the water or solution that have brought this about be assumed to have been low or moderate. When the process has proceeded far enough to cause the production of a clear or pure hydrous glassy substance (sometimes cryptocrystalline) then the product can be termed chlorophaeite, provided it has a chemical composition falling within the range of composition exhibited by substances already definitely accepted as chlorophaeite, the two limits of which are roughly indicated by Heddle's original analysis of the minerals from Rum and the Giants' Causeway respectively.

It seems unlikely that this discussion can be brought to a more satisfactory conclusion until the pure mineraloid of a selection of the occurrences regarded by Dr. Peacock as true palagonite has been isolated and analysed. If as a result of analysis of such pure material from two or three localities it can be shown that palagonite does not fall within the range of composition shown by substances definitely accepted as chlorophaeite, then it will be necessary to agree that palagonite is a different substance, though even then it may belong to the chlorophaeite series.

Recent Discoveries of Dinosaurs in India.

By C. A. Matley, D.Sc., F.G.S.

UNTIL the last few years the Asiatic Continent has yielded remarkably few remains of Dinosaurs. The recent discoveries in Mongolia have revolutionized our knowledge of their distribution in Northern Asia, and now much new information has come to light from collections made from the Cretaceous rocks of India.

The older collections of dinosaurian remains from India have been disappointingly meagre. From the pre-Cretaceous rocks

we have only *Epicampodon indicum* (Huxley) from "Lower Trias" and a doubtful Stegosaurian (*genus non det.*) from the "Upper Trias".[1] In the Cretaceous rocks the carnivorous Dinosauria were represented merely by a single tooth from the Trichinopoly district (attributed to *Megalosaurus* sp.) and another tooth from Takli, Nagpur (*Masso-spondylus rawesi*). Of the non-carnivorous dinosaurs the only specimens described were a few bones from Jubbulpore and Pisdura which Lydekker in 1877 and 1879 assigned to a new genus of sauropods, *Titanosaurus*, with two species, *T. indicus* from the Lameta formation at Jubbulpore and *T. blanfordi* from Pisdura. The validity of this genus, which has since become the type-genus of the family Titanosauridae, practically depends on two caudal vertebrae from Jubbulpore, as an imperfect femur described by Lydekker as a cotype has been shown to have come from a different bed in the Lameta formation, and is now being assigned to a different genus. The attribution of dinosaurian bones from other parts of the world (Europe, South America, Madagascar) to Lydekker's genus has therefore been hitherto based on the characters of the caudal vertebrae in the absence of any further knowledge of the Indian reptile.

In October, 1917, during a visit to Jubbulpore, I was fortunate in discovering a considerable number of bones in the Lameta Beds of Bara Simla Hill, among which representatives of sauropodous and theropodous dinosaurs were at once recognized. With the assistance of the Director of the Geological Survey of India, to whom I reported the discovery and to whom I presented the material, excavations were made and large collections sent to Calcutta, where I was able to give them a preliminary examination. A search in 1919, along the Lameta outcrop in the Jubbulpore district, added a large femur from near Silondi to the collection, and in 1920 I made a collection from Hislop's locality at Pisdura. A short notice of these discoveries was published in 1921 in a paper dealing chiefly with the local stratigraphy.[2]

Most of the sauropod bones from Jubbulpore and the femur from near Silondi have been retained in the Indian Museum, Calcutta, but the remainder of the collection was sent to London, by arrangement with Sir H. H. Hayden, for further study. Sir Arthur Smith Woodward very kindly offered to provide storage room and facilities for examination at the British Museum (Nat. Hist.), and the same facilities have been generously continued by the succeeding Keepers of the Geological Department, Dr. Bather and Dr. Lang. The late Dr. C. W. Andrews was of great help in the early stages of the

[1] "Synopsis of Indian Pre-Tertiary Reptilia and Amphibia": *Pal. Ind.*, 4, i, pt. 5, 1885, 37. Recently H. C. Das-Gupta has described a tooth from the Panchet Beds as *Teratosaurus* (?) *bengalensis*. *Jour. As. Soc. Bengal*, N.S., xxiv, 1928, 476–8.

[2] C. A. Matley, "On the stratigraphy, fossils, and geological relationships of the Lameta Beds of Jubbulpore": *Rec. G.S.I.*, liii, 1921, 152–7.

study of these fossil bones, a study which was seriously interrupted by prolonged residence and journeys abroad, and later Dr. W. E. Swinton has been good enough to continue that help. The preparation of the bones for descriptive purposes has been a troublesome and lengthy task, in which I have been fortunate in having the skilled services of Mr. F. O. Barlow and Mr. L. E. Parsons. The examination of all the material has at last been brought to a conclusion, but only by the co-operation of Professor F. von Huene, of Tübingen, who, having expressed his great interest in the collections, very kindly undertook the major part of the work himself, by describing the whole of the carnivorous dinosaurs and by joining with me in the account of the Sauropoda. I had already (in 1923) described the only example of an armoured dinosaur found in the collection.[1]

A full description of the bones has been written, and will in due course be published in *Palaeontologia Indica*, but in view of the great extension of our previous knowledge of Indian dinosaurs which the new collections have revealed, I have thought it desirable, with the consent of the Director of the Geological Survey of India, to anticipate the detailed description by a brief account of the chief results of the study of these specimens.

Jubbulpore.

The new collections were chiefly obtained from the Lameta Formation at Bara Simla Hill, the stratigraphy of which was described by me in 1921 (*op. supra cit.*). The Lameta Beds of Jubbulpore, which rest on the Jabalpur Group and are capped by Deccan Trap, vary in thickness from 70 to 160 feet and are divisible into the five lithological zones shown in the accompanying columnar section (Fig. 1). The age of the formation is not definitely fixed, but has been considered to range from Albian or Cenomanian to Turonian.

The remains at Bara Simla Hill were found at three horizons, as shown in Fig. 1, but those obtained from the conglomeratic top of the Main Limestone were so broken and rolled that they were not worth collecting; it was noted, however, that they were sauropodous. The *Sauropod Bed*, so-called because the remains are chiefly (but not exclusively) sauropodous, lies about 4 feet above the rolled bones, and the *Carnosaur Bed*, in which the main mass of carnosaurian bones lay, is a local "pocket" crowded with bones at the top of, and descending several feet into, the Greensand Zone, immediately under the Main Limestone. The bones from this mass are not exclusively carnosaurian, as parts of a stegosaur (*Lametasaurus indicus* Matley) and a few sauropod and chelonian

[1] "Note on an Armoured Dinosaur from the Lameta Beds of Jubbulpore": *Rec. G.S.I.*, lv, 1923, 105–9.

remains have been found in it. Unfortunately, although parts of a considerable number of individuals have been obtained, the bones do not occur at any of the horizons in their natural relative positions, and in only a few cases can they be definitely regarded as associated parts of the same reptile. The difficulty of correlating them is therefore great and is increased by the fact that, not only are many parts of the skeletons fragmentary or missing, but we are dealing with hitherto unknown genera and species of Indian dinosaurs.

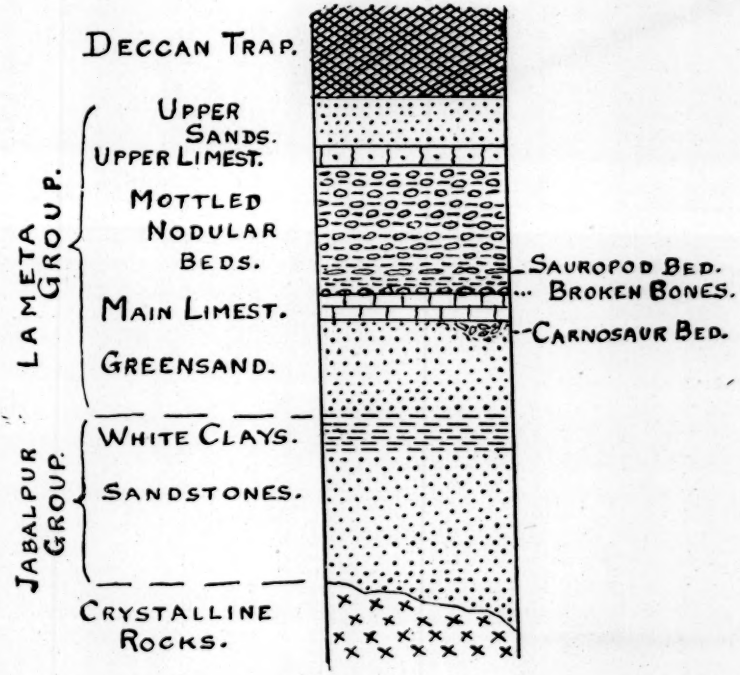

FIG. 1.—Columnar section of the strata, at Jubbulpore, showing the fossiliferous horizons.

Consequently, it has been impossible to assign much of the material to definite individuals or species.

Sauropod Bed.—From this bed has been obtained further parts of *Titanosaurus indicus* Lydekker, including a left tibia and a right fibula. A larger Titanosaur is assigned by von Huene to a new species of his recently described South American genus *Antarctosaurus*; it is represented by the hinder part of a skull (with brain cavity), a squamosal, hyoid, ribs, a caudal vertebra, haemapophyses, a pair of scapulae, right humerus, right radius, right ulna, and part of a

pubis or sternal plate. This was a very large reptile, the scapulae being over 5 feet in length, and the chevrons (haemapophyses) measuring up to 18½ inches. Some teeth and other parts of carnivorous dinosaurs were also found in this bed.

Carnosaur Bed.—This bed is a mass of bones that occurs as a pocket at the top of the soft green sandstone of the Greensand Zone. A large collection obtained from it contained a varied assemblage of dinosaurs, chiefly carnivorous. The number of individuals must have been considerable, as lower jaws alone show that a minimum of five Allosaurids are represented, apart from other families found. The Coelurosauria are recognized for the first time in India, the Coeluridae and Ornithomimidae being both well represented, and perhaps also the Compsognathidae. In addition are a stegosaur, a few sauropod remains and some parts of chelonians. The sauropod (Titanosaur) bones include a large and nearly complete left maxilla of the shape of *Camarasaurus* and *Brachiosaurus* but of unknown genus, an incomplete right femur (cf. *Titanosaurus*) and the centrum and spine of a dorsal vertebra of a Titanosaur.

The stegosaur *Lametasaurus indicus* was described by me in 1923 from the sacrum, ilia, and left tibia, and parts of the armour found in this bed. It was expected that other parts of the skeleton, perhaps the hinder part, would be found, but no more skeletal bones have been recognized, only additional scutes. Dr. Barnum Brown, who visited Jubbulpore a few years ago and made further excavations into this bed, has obtained a large plate, identified by him as the terminal caudal plate of "a new armoured dinosaur". It was on exhibition at the American Museum of Natural History, New York, when I visited it last year. In view of the fact that the only armoured dinosaur found in India came from this very bed (and probably only a few feet away from the plate), it is likely that this plate is another fragment of *L. indicus*, and indicates that, like the Canadian *Ankylosaurus*, this reptile carried a club at the end of its tail. Some of the undescribed scutes are reminiscent of the rings of heavy plates that protected the neck of the Canadian *Palaeoscincus*.

Carnosauria.—Among the numerous Carnosaurian remains, several portions of skulls are of special importance, and from these it has been possible to found two new genera of Allosauridae, chiefly on the characters of the skull roof, supplemented in one case by a brain cast. Other parts of Carnosaurian skulls and skeletons are described but are left unnamed as it is uncertain to which, if either, of these genera they belong. The material includes several maxillae, a pair of premaxillae, lower jaws, about sixty teeth belonging to two species, haemapophyses, abdominal ribs, a number of femora, two tibiae, and numerous other bones. The foot bones include the remains of at least three kinds of Carnosauria. Considered as a whole, all probably belong to the family Allosauridae, and to at least three species. Dr. Barnum Brown's collection from this bed includes a

pair of premaxillae, maxilla, lower jaw, some vertebrae and other parts, all of which seem to belong to the species in the Indian collection.

Coelurosauria.—A large number of remains belong to the Coelurosauria. Both the Coeluridae and the Ornithomimidae are well represented, and a member of the Compsognathidae may also be present. Of the Coelurids four new genera are recognized, chiefly from the characters of the cervical and dorsal vertebrae; one of the forms is allied to *Dryptosaurus* and is even larger than the North American species of that genus. Of the Ornithomimids two species of a new genus reminiscent of the North American *Ornithomimus* and *Struthiomimus* have been founded on dorsal vertebrae. A large number of Coelurosaurian bones, including many parts of the pes and manus, are described but left unnamed, as they cannot be distributed to the different species.

Other Beds in the Jubbulpore District.

The "imperfect femur" described by Lydekker as a co-type of *Titanosaurus indicus* seems to have come from a siliceous part of the Greensand Zone at Jubbulpore; it appears to be too slender for the genus *Titanosaurus* and is much more like in its proportions the Patagonian *Antarctosaurus wichmannianus*. An incomplete humerus in a similar matrix from the same zone at Jubbulpore likewise approaches the shape of the same Patagonian species, and they are both diagnosed as "cf. *Antarctosaurus* sp.". The large femur from near Silondi is a much broader femur, and is attributed to "cf. *Titanosaurus indicus*".

Pisdura.

In January, 1920, during a short visit to Pisdura, in the Chanda district of the United Provinces, I picked up a number of sauropod bones, besides coprolites and parts of chelonians, from the surface of a ploughed field at Hislop's locality. They consist of vertebral centra and broken limb bones, and as it is evident that many of the fractures have taken place since fossilization, due to the bones having been brought to the surface by ploughing, better specimens ought to be obtainable by excavations into the subsoil. Professor von Huene has examined and described the sauropod material which all belongs to Titanosaurs, and has identified cf. *Titanosaurus indicus*, *T. blanfordi*, *Antarctosaurus* (?) sp., and cf. *Laplatasaurus madagascariensis* (Depéret). A few feet above this dinosaur bed the characteristic "Inter-Trappean" mollusc *Bullinus prinsepii* (Sow.) is found in abundance (*vide* my 1921 paper *ut supra cit.*, p. 161).

Trichinopoly District.

Reference will now be made to some recent discoveries in the Cretaceous beds of South India near Ariyalur in the Trichinopoly

District. A " bone-bed " containing very poorly preserved remains was discovered by Blanford, during the survey of 1857 to 1860, to be intercalated among fossiliferous marine strata now known to be of Maestrichtian age, but only a tooth of a carnivorous dinosaur (attributed to *Megalosaurus* sp.) was identifiable. As no record of further discoveries from this bed was known, I arranged, with the co-operation of the Director of the Geological Survey of India, to make a search during my visit to India in February, 1925. The results (which are recorded in a paper published in 1929) [1] were disappointing in that although a number of scattered bones were found, some of them being of great size, up to 60 inches in length, they were all too poorly preserved for identification or removal. They were, however, not carnosaurian, and belong very probably to the sauropoda, and the fauna here is therefore more varied than was previously known.

Not long after my visit the geology and zoology students of Mysore University examined the Ariyalur beds, and found a number of bones in better preservation, of which a preliminary description was given in 1927, the authors reaching the conclusion that " the fossils must belong to a dinosaur reptile hitherto undescribed ". [2] From their account it is clear that a carnivorous dinosaur is included in their collection, which I understand has now been supplemented by later additions. I am informed that the specimens are being carefully studied by the Professors and others on the staff of Mysore University, who are engaged in the preparation of a monograph on the Cretaceous rocks of the Trichinopoly area, and it is to be hoped that a full account of this important material will soon be available.

Summary of the Cretaceous Dinosaur Fauna.

The result of the examination of these collections is that the list of Indian Cretaceous dinosaurs now stands as under :—

Sauropoda.	*Localities.*
Titanosaurus indicus	Jubbulpore.
Cf. *Titanosaurus indicus*	Silondi, Pisdura.
Cf. *Titanosaurus* sp.	Jubbulpore.
Titanosaurus blanfordi	Pisdura.
Antarctosaurus nov. sp.	Jubbulpore.
Antarctosaurus (?) sp.	Pisdura.
Cf. *Antarctosaurus* sp.	Jubbulpore.
Cf. *Laplatasaurus madagascariensis* (Depéret)	Pisdura.
Carnosauria.	
Fam. Allosauridae.	
Two new genera and species	Jubbulpore.
Many parts not yet assignable to species	Jubbulpore.

[1] " The Cretaceous Dinosaurs of the Trichinopoly District and the Rocks associated with them " : *Rec.G.S.I.*, lxi, 1929, 337–49.

[2] C. R. Narayan Rao and B. R. Seshachar, " A short Note on Certain Fossils taken in the Ariyalur Area (S. India) " ; and L. Rama Rao, " Notes on the Upper Cretaceous Rocks near Ariyalur, S. India " : *Half-Yearly Jour. Mysore Univ.*, i, No. 2, 144–52 and 153–7.

COELUROSAURIA.	
Fam. (?) Compsognathidae.	
New genus and species . . .	Jubbulpore.
Fam. Coeluridae.	
Four new genera and species . .	Jubbulpore.
Fam. Ornithomimidae.	
One new genus with two species . .	Jubbulpore.
Many skeletal bones of Coelurosauria the correlation of which with named species is uncertain . . .	Jubbulpore.
STEGOSAURIA.	
Lametasaurus indicus	Jubbulpore.

The collections from the Central Provinces have therefore added eleven genera, of which nine are new, to the former scanty list of Indian dinosaurs.

AGE OF THE FAUNA.

At my request, Professor von Huene has added to the forthcoming memoir for *Palaeontologia Indica*, a chapter dealing with the age of the fauna and the light it throws on Cretaceous palaeogeography. The presence of cf. *Laplatasaurus madagascariensis* at Pisdura suggests correlation with the Madagascar deposits containing that species, the age of which, according to Lemoine, is "Turonian to Emscher". This form and also *Titanosaurus blanfordi* have not been found at Jubbulpore, although the two faunas are otherwise closely related and *T. indicus* and *Antarctosaurus* may possibly occur in both. He thinks, therefore, that the dinosaur horizon at Jubbulpore is somewhat lower than at Pisdura, and takes the former to be Turonian and the latter to be lowest Senonian or Campanian. If this view is accepted for Pisdura, then *Bullinus prinsepii*, which occurs a few feet above the dinosaur bed there, is older than it has been previously considered to be. The assignment of the Jubbulpore horizon to an age not high in the Upper Cretaceous is supported by the presence of the numerous Allosaurids, the occurrence of Coeluridae and Compsognathidae, and the presence of *Lametasaurus*, a not very highly specialised Nodosaurian.

PALAEOGEOGRAPHY.

The connection of Peninsular India with Madagascar in Mesozoic time as part of the continent of Lemuria is well known, and the occurrence of cf. *Laplatasaurus madagascariensis* at Pisdura is not astonishing, but it seems really remarkable that all the three Cretaceous sauropod genera of India—*Titanosaurus*, *Antarctosaurus*, and *Laplatasaurus*—should be found so far away as Patagonia and other parts of South America, although they occur there in younger beds (Upper Senonian = Maestrichtian). Von Huene has already discussed in other papers the communications between South

America and Asia in Mesozoic time and has come to the conclusion that in Cretaceous, and especially Upper Cretaceous, time South America was connected to south-eastern Asia by way of Antarctis, the Australian region and the Sunda Archipelago, and he regards the close relationship of the dinosaur faunas of these two distant regions as additional supporting evidence of such a connection.

Concluding Remarks.

It is a great pity that up to the present nothing more than scattered bones of Indian dinosaurs have been found. A few articulated skeletons, or even portions of skeletons, would be of wonderful help in allowing a correlation of many of the unnamed bones of the Jubbulpore collection. There is still hope of obtaining such, as the Lameta outcrop extends for some hundreds of miles as a narrow fringe to the Deccan Trap, and much of it has never been examined in any detail, certainly not with a view to finding fossils. Now that it is known that many kinds of dinosaurs roamed over that country in Cretaceous times the prospect of further and probably better finds coming to light if systematic search is made seems good. The result of a fortnight's work in 1919 in the Jubbulpore district up to a distance of 40 miles north-east of the city showed that remains occurred sparingly at a number of localities, though they were mostly too poor to be collected (*loc. cit.*, pp. 156–7).

The Turoka Series of East Africa and the Nachipere Series of Nyasaland.

By F. Dixey, O.B.E., D.Sc., Director of Geological Survey, Nyasaland.

In The Geological Magazine of November last, Dr. John Parkinson,[1] referring to my paper on the "Geology of the Lower Shire—Zambezi Area",[2] advanced reasons for regarding the Nachipere Series of Nyasaland as being younger than the Turoka Series of East Africa.

For many years the crystalline rocks of southern Nyasaland were regarded as a group or groups of completely altered rocks in which little or no evidence remained as to original characters, although on general grounds they were considered in part at least, as representing sedimentary rocks. Within the last few years several localities have yielded undoubted evidence of the persistence of original clastic structures in these rocks, and because of the interest of these observations it is perhaps possible that

[1] "A Note on the Turoka Series of East Africa, with a reference to the Nachipere Beds of Nyasaland," Geol. Mag., LXVII, 1930, 521.

[2] Geol. Mag., LXVII, 1930, 49.

undue emphasis has conveyed the impression that such structures are almost everywhere observable. Such is not the case, however, for in general the older rocks of southern Nyasaland are completely recrystallized; sometimes it is just possible to recognize clastic structures, and only rarely, as in Nachipere Valley, are they readily apparent. In every case where the sedimentary character of the rocks is evident, careful examination has failed to reveal anything in the nature of a break between these rocks and the enclosing crystallines; there is always a passage so gradual that it is not possible to say just where the sedimentary character is lost and where complete recrystallization begins. The Nachipere conglomerate for example is largely recrystallized, and the outlines of the pebbles are often blurred; when the rock is followed down the bed of the Nachipere the blurring and deformation increase until the original characters are lost in complete recrystallization. The same remarks apply to the associated arkoses and other coarse sediments, in which small quartz pebbles are sometimes clearly recognizable. The finer-grained and more argillaceous beds rarely appear other than as mica-schists and these rocks are sometimes interbanded with coarser beds in which clastic structures are still more or less clearly visible. In general the banded rocks interfoliated with the crystalline limestones afford no indication as to their origin, but in one case, in the Nachirenda valley, Port Herald Hills, the common white crystalline limestone is interbanded with altered pebbly grits and arkoses. Moreover, in the country to the west crystalline limestones, graphitic schists, and indeterminate micaceous and hornblendic schists are interbanded with quartzites and hard quartzitic grits.

As regards the effect of the intrusions on the sediments, this of course is more apparent in areas where the sediments had previously undergone relatively little metamorphism. I do not agree with Dr. Parkinson's suggestion that in general the metamorphism has been inaugurated by late plutonic intrusions. In parts of southern Nyasaland, particularly in the upper Mwanza area, and near the head of the Bwanje Valley, Mr. C. B. Bisset has observed well-foliated igneous rocks in intimate association with the metamorphosed sediments. These rocks are composed chiefly of felspar and quartz, with dark ferromagnesian minerals, particularly biotite, and appear to bear a close resemblance to the well-foliated biotite-gneiss described by Dr. Parkinson from Masisi. The rocks are cut by large intermediate intrusions, generally unfoliated, which are readily distinguishable from the gneisses and appear to be of much later age.

If for the moment we neglect the localities in which the sedimentary character persists, then the older rocks of southern Nyasaland bear many resemblanes to those included by Dr. Parkinson within his Turoka Series. He refers particularly to the "kyanite, sillimanite, garnet, and scapolite schists and gneisses, and crystalline

limestones " of Morogoro—which are comparable with the rocks of parts of southern Ncheu District (*Nyasaland Geol. Surv. Ann. Rep.* 1929, p. 7) ; to the " well-foliated biotite-gneiss, its micaceous bands alternating with others of a more quartzo-felspathic nature " of Masisi—rocks familiar in many parts of Nyasaland ; and the " crystalline limestones with felspathic quartzites, both containing graphite, together with garnet-bearing rocks " on the motor road to Newala—which are clearly comparable with certain of the rocks in and west of the Port Herald Hills referred to in an earlier paragraph.

Dr. Parkinson's objections to the general equivalence of the Turoka Series and the Nachipere Series all rest upon the variability in the metamorphism of the latter group. Yet he admits the possibility of a lower degree of metamorphism in north-east Suk than elsewhere, as well as the possible occurrence of clastic structures at other places in the Turoka Series, although in an insignificant proportion of the rocks as a whole. Yet if they may be present even to this slight extent in the type area, why may they not be present to a much greater extent in rocks of the same age at a distance of 500 miles or more to the south ? One of the main difficulties in the correlation of the older rocks of central Africa has proved to be the great variation in the metamorphism of any one group when followed from one area into another.

Dr. Parkinson draws a comparison between the invasion of the " Basement Schists " of Northern Rhodesia and the early history of the oldest rocks of Kenya. In the course of the International Geological Congress of 1929, I had the opportunity of examining a number of exposures of these ancient schists, in which I was particularly interested. The great variability in the metamorphism and deformation shown by these schists, and the lithological character of the rocks themselves, left little doubt in my mind as to the general equivalence of these rocks with the Mafingi Series of north-eastern Rhodesia and the adjacent part of Nyasaland ; and if these schists are comparable with the Turoka Series then so are the older metamorphosed sediments of northern and southern Nyasaland.

In the paper cited by Dr. Parkinson, I have compared the Mafingi Series with the Lomagundi System of southern Rhodesia, and an examination of specimens from the two groups supports the comparison. The Lomagundi System also shows considerable variations in metamorphism within short distances ; moreover, the underlying " Basement Schists " (of southern Rhodesia), of far greater age, exhibit similar variations, and they include locally sandstones and grits that appear to be normal sediments, and in the hand specimen show little if any sign of metamorphism. They are none the less probably at least as ancient as the Turoka Series, and are much less distant from the Nachipere Series than the latter is from typical outcrops of the Turoka Series.

Accordingly, variations in the character and extent of metamorphism would not appear to afford sufficient ground for considering the Nachipere and related rocks as younger than the Turoka Series.

It is of course necessary to admit that further work may throw much additional light on the characters and relations of the older rocks of southern Nyasaland, and that these rocks may be found to be divisible into groups of different ages. Moreover, I consider there is good ground for regarding the Nachipere Series as being much younger than the Basement Schists of southern Rhodesia, which are considered to be of Archaean age; if therefore Dr. Parkinson's view as to the age of the Turoka Series proves to be correct, then the Nachipere Series may, as he suggests, be younger than the Turoka Series. But while the correlation of these series with other ancient African systems remains in doubt, and until information as to an additional ancient group or groups in southern Nyasaland is forthcoming, I am inclined to regard the available evidence as indicating that the Turoka Series and the Nachipere Series may be of similar age.

Summarizing the preceding paragraphs: the published descriptions point clearly to at least some of the older rocks of southern Nyasaland as representing the Turoka Series; locally within this part of Nyasaland clastic sediments are developed, and detailed investigation has shown that by means of progressive metamorphism these sediments pass into completely recrystallized schists; this group of sediments and their metamorphic derivatives have been described under the name of Nachipere Series; they differ from the Turoka Series essentially in the presence locally of only slightly metamorphosed members; accordingly, and in view also of the variability in the metamorphism of the older rocks of this part of Africa, it is not improbable that the Nachipere Series represents the Turoka Series.

Finally, although I am unable to accept all Dr. Parkinson's conclusions, and while I regard his note as affording additional evidence for the view that the Turoka Series extends as far south as Nyasaland, yet I welcome his criticism, since it is largely by this means that we may ultimately arrive at correct conclusions as to the correlation of the older rocks of Africa.

REVIEWS.

EAST YORKSHIRE: A Study in Agricultural Geography. By S. E. J. BEST. Longmans Green.

MR. BEST has produced an interesting account of the agriculture, soils, and economics of the East Yorkshire area. The book is well illustrated with many diagrams, maps, and photographs of the district. It should prove useful to students in local agricultural institutes, and to schools where geography is taught with a rural bias.

Some of the author's views, especially those expressed in the last section—"The East Riding of To-day"—are open to criticism, and the work is somewhat marred by the colloquial style in which it is written, but Mr. Best has produced a general survey of his area which contains a useful store of information both for students and for soil chemists. The book has no index or bibliography, which detracts greatly from its value.

UNIVERSALDREHTISCHMETHODEN. EINFÜHRUNG IN DIE KRISTALLOPTISCHEN GRUNDBEGRIFFE UND DIE PLAGIOKLASBESTIMMUNG. By M. REINHARD. pp. 120, with 5 plates and 49 text-figures. Basel: Wepf and Co. 1931. Price: 8*s*. 6*d*. unbound, 10*s*. bound.

THE adaptability of the universal stage towards solving many different optical problems must soon impress itself on anyone acquainted with its practical use. Professor Reinhard makes an even wider claim in urging its application in teaching the elements of crystal optics. Written from this standpoint, his book is frankly intended only as an introduction from which the worker is recommended to proceed to more advanced works such as that of Berek. Part I includes the elementary theory, and a very full description of the practical adjustment and use of the apparatus. Part II deals with its application to the determination of the plagioclase felspars from the orientation of the indicatrix: the five reference stereograms are contained in a pocket in the cover, and include projections both on optical and on crystallographic planes. The author's enthusiasm for the subject and the clarity of his writing make the book one well calculated to advance the aim expressed in the preface, "to attract new friends to the universal methods of that ingenious investigator, Fedoroff."

F. C. P.

DIFFICULTIES OF THE EVOLUTION THEORY. By DOUGLAS DEWAR. pp. viii + 192. Edward Arnold & Co., London, 1931. Price 12*s.* 6*d.*

IN these days when the hypothesis of organic evolution is almost universally accepted, not only by men of science, but by the general educated public, it will come as a surprise that a naturalist who has done good observational work in ornithology and other fields of zoological study should have written a book adopting a "provisional hypothesis of special creation supplemented by a theory of evolution" to account for the phenomena presented by living organisms. Yet we may call to mind that the late William Bateson said somewhere not long before he died, that there is no *direct* proof of the truth of organic evolution. It was, he said, a matter of faith, "the substance of things hoped for, the evidence of things not seen." But he remarked, further, that biologists *are* evolutionists because it is only on this theory that a vast array of facts can be co-ordinated or brought into relation and to that extent "explained". On Mr. Dewar's views, however, "a new organism may originate in one or other of two ways, by a slow process of evolution or a rapid process of creation." Thus, he is sufficiently impressed by the evidence in support of the doctrine of descent to admit that this doctrine is probably true as applied to a large number of organisms. On the other hand he is equally conscious of the difficulties; and by difficulties he apparently means those facts which cannot at present be interpreted, or readily interpreted, on the mutation hypothesis. Much of this adverse evidence is of a kind that has previously been presented by opponents of the evolution theory—the negative results obtained by experimental breeders, the absence of nascent organs and structures, the anomalies in geological distribution both in present and in past times, and the lack of fossils testifying to the gradual origin of all peculiar types. It must be admitted that the author makes some shrewd criticism, as where he adduces data for mammals indicating that the imperfection of the geological record is not nearly so great as it is usually said to be. In fact, Mr. Dewar makes out a better case than might have been expected, but some of his criticism is valid only against the doctrine of natural selection, and is in no way opposed to the theory of descent. As with certain other writers, great stress is laid on the abrupt appearance in the geological succession of new families and larger groups, as manifested by the forms which have been found fossil.

F. H. A. M.